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FOURTH CADMAN MEMORIAL LECTURE*

WINNING PETROLEUM

By C. A. P. SOUTHWELL,† C.B.E., M.C. (Past President)

WHEN the Council of the Institute of Petroleum asked me to deliver the Fourth Cadman Memorial Lecture, I was deeply appreciative of the honour it conferred, particularly in view of its international character. To me, there is the additional gratification, which only a student of Professor Cadman could feel, that I am to receive the " medal of the master."

At this present time, each phase of the petroleum industry is an industry in itself. Each has experienced its own evolution, for there has not only been an incredibly rapid advance in the winning of the raw material and the distribution of petroleum, but there has also been a great expansion in the uses to which it has been put in the service of man.

On this occasion of the Fourth Memorial Lecture, I propose to discuss the problem of obtaining the raw material—the crude oil which is the basis of our industry—and to link it with the pioneer work of Lord Cadman.

Lord Cadman's earliest experience was in mining engineering practice in the sedimentary rocks, and this is an important factor when considering the influence it had on his earliest activities in oil. He studied the problems, which still confront us today, as an observer of the sedimentary rocks on the surface, under the ground, and in the laboratory. His approach to the petroleum problem had a bias of scientific exploration and followed the orderly development of the mineral deposit. This approach, as far back in our history as fifty years ago, revealed that he was a true pioneer, a fact which the historical records of our industry appear to have overlooked.

I should like to digress at this point and refer to a period 100 years ago in the history of Trinidad, some fifty years before Cadman's appointment there in 1904, because it throws some light on British thought at that time, and on the knowledge which was available to him.

In 1855, some four years before the discovery of the Drake well—which is regarded historically as the start of the petroleum industry in the U.S.A.—Sir William Molesworth, Secretary of State for the Colonies, addressed a letter to the Governor of Trinidad, Sir Charles Elliott, K.C.B., R.N., as follows :

> " I have now to acquaint you that the Lords Commissioners have given their assent to the following arrangements : A fully competent person to be selected by His Majesty's Government who shall in the first place undertake a general survey of the economic geology of Trinidad. I shall lose no time in communicating with Sir Roderick Murchison (Director-General of the Government School of Mines in Jermyn Street) as to the selection of a gentleman for this service."

As a result, Messrs G. P. Wall and J. G. Sawkins were appointed to undertake a two-year investigation of the economic geology of Trinidad. Their report reads, in part, like an up-to-date review of the world's oil-producing regions, and this was the only published information concerning Trinidad available to Cadman when he took up his appointment there, some fifty years later.

In reviewing the asphaltic oil deposits and the live oil and gas seepages in Trinidad, Wall compares them with similar occurrences at Baku. He refers to Texas and quotes the existence of a pitch lake 100 miles from Houston, similar to that in Trinidad, but smaller. He mentions Persia, stating that bituminous petroleum and naphtha are found in all countries bordering on the Tigris and lower Euphrates : while he states that in Arabia, bitumen and petroleum are very abundant in strata composed of oceanic matter.

This is an instance of a very early British geological approach to the oil problem, and yet the wide application of the science of geology to the petroleum industry did not gain its real momentum until 1914, some sixty years later.

Whilst Wall was undertaking his survey in Trinidad in 1857, an American Company erected a distillation plant there to distil pitch for lamp oil and lubricating oil for export to the U.S.A., proof that even at that time American and British co-operation existed.

I have referred to those early days because, in the 1920s, when I was Petroleum Technologist to the Government of Trinidad, I had ample opportunity of seeing from the records there how this original work influenced many of the recommendations which Lord Cadman subsequently made. He was at that time already imbued with the concept of unit operation and scientific development.

The most significant development in the oil industry

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* Delivered in the Lecture Hall of the Royal Institution on 17 November 1954.

† Managing Director, Kuwait Oil Company Ltd.

of recent years has been the increasing demand throughout the world for petroleum products and, as an introduction to the vital question of obtaining the raw material, it might be as well to refer briefly to what the future needs are likely to be.

For all practical purposes, crude oil production commenced at the beginning of the twentieth century. In the year 1900 the world's requirement was a production of only 50,000 tons. By comparison, it has been estimated that 685 million tons will be required in 1954. There has always been an inclination to under-estimate future demands, and this tendency continues today.

In looking to the future, many new factors have to be taken into consideration, for oil is becoming more and more important to the social progress of all countries—not only in transport, which on land, sea, and air consumes more than half the world's production, in the home and in industry, but also as a raw material for chemical processes. Despite this, however, I do not think that any statistician would seriously consider the possibility of the present rate of increase, of 5 per cent per annum, continuing for the next fifty years.

Taking into account increases in population and the fuller use of oil in all countries, the great majority of which now have such a low level of oil consumption that they offer immense fields for expansion, I suggest that a more probable estimate of the increase in demand during the next fifty years would be 3 per cent per annum. This would mean that just over 2000 million tons a year would be required by the end of the century—some three times the present world production.

With this in mind as a broad basis, consideration can be given to some of the problems which would be involved in providing these large quantities of oil.

The key to future search is in the fundamental knowledge of the origin and conditions under which oil is found. It is desirable, therefore, to review the enormous amount of new knowledge brought to light in the last thirty years of development. This new knowledge, and the more advanced present-day scientific thought, must form the basis of finding new oil deposits.

When I review the changes which have taken place since the time when Cadman was a Professor of Oil Technology, I find that, whilst a considerable amount of detail has been filled in, there is relatively little change in the principles which were then accepted. It was known then to the petroleum industry that it was dealing with a mineral fuel, largely formed from organic matter in marine sediment and widely disseminated in sedimentary rocks.

Studies of the main petroliferous provinces, and their productive oilfields, show that they are normally located in thick basins of sediments fringing the large land masses in gently folded areas. These regions are relatively young, geologically speaking, because oil stored in the earth rarely survives a geological cycle.

There appears to be little difference of opinion in geological circles regarding the marine organic source material of petroleum—nor are there any significant differences of opinion on the compaction processes during which the oil starts to form. Such gaps in our knowledge as whether catalysts, which are so important in refining, play any part in the compaction processes, can only be filled today in the laboratory.

The main change is that the relatively thin veneer of sediments covering the greater part of the surface of the earth is providing, as new sources are found, every possible type of oil reservoir, both structurally and in regard to rock texture. This has widened considerably, not only the scope of the search for oil, but views previously held on the extent of the reserves yet to be found.

The petroleum industry would appear to have reached a stage when any sealed arrangement of porous rock in the sedimentary series is of interest. As oil has been found in granite voids, even igneous rocks cannot entirely be ruled out as possible reservoirs, under restricted and special conditions.

Reports made, some little time ago, to the United Nations Scientific Conference on Fuels, stated that estimates of undiscovered reserves of oil were of the order of 500 times the current annual world consumption, *i.e.* over 300,000 million tons. Such estimates as this reflect improvements in technical development and the optimism of geological thought, rather than increases based on scientific data. The same report continues, "As ideas have developed, estimates have increased and may be expected to continue to increase in the future." We might, with profit, examine this view.

A high proportion of the 20 million square miles of sedimentary rocks lying on the earth's surface has received, by now, a preliminary geological examination, either on the ground or by air-photography. As a result of this examination, opinions have been expressed that as much as one-third of this area can be considered as possible territory for oil accumulations. In reviewing the question of reserves, Pratt, Lees, Weeks, and other eminent authorities on the subject agree that an area of up to 6 million square miles of sedimentary land surface may be worth prospecting for oil accumulations.

To this figure must be added at least an equal area comprising the continental shelf, lying off shore of the land sedimentary rocks, resulting in a total prospecting surface of not less than 12 million square miles.

Taking this vast area as a basis for their calculations, American petroleum geologists have approached the problem from a global viewpoint and estimated the area of sedimentary rocks in cubic miles. They have then applied an average figure of oil content per cubic mile of sedimentary rocks, based on experience in the U.S.A., which has been very widely tested by the drill. This figure tends to vary according to the optimism of the scientist concerned, and may be as high as 12,000 tons of oil per cubic mile. By using such statistics as a basis of computation, it may be

deduced that the undiscovered oil remaining in the world today amounts to between 100,000 and 200,000 million tons.

American scientists have collated the information made available to them as a result of tests carried out in the U.S.A., where at the present time 90 per cent of the world's drilling effort is employed and where some 12,000 exploration wells were drilled in 1953. This extensive accumulation of data is a most valuable background to any global assessment, particularly from a country which in the past has provided over 60 per cent of the world's total production of oil.

This method of applying the American average to the rest of the world might be regarded as lacking scientific basis and dangerously removed from the truth. But this is not the case. Our American friends have a great capacity for reducing difficult problems to simple mathematics. They have built up their knowledge from information obtained from many parts of the world, and the statistics provided have been tested by geological opinion in many countries. In all probability, these practical men of science, on whom the industry relies so much, will do even better yet, for the discovery of oil continues to be a geological enterprise.

Lord Cadman himself was averse to expressing opinions on oil reserves, but, in talking to the Royal Society of Arts in 1920, he said that it was difficult for him to accept the pessimistic views, then held by many, that reserves of oil in the U.S.A. were being rapidly exhausted. To quote his own words, "Oil is not a commodity that can be easily measured and no estimates, however conservative, can be strictly relied upon."

Until more fundamental research has been undertaken on sedimentation, figures for unproved oil reserves—which, after all, are only thoughts in the minds of our petroleum geologists—must be treated with caution. There is, however, a much clearer picture before the industry today than there was in 1920.

Such estimates, large as they are, would appear to be alarming for the twenty-first century, bearing in mind that today petroleum provides about one-third of the world's energy requirements, were it not for the knowledge we have of the vast quantities represented by the alternative sources which exist. For the present, such other sources of energy as synthesis of shales and tar sands extraction—quite apart from the rapidly approaching possibility of atomic energy for power generation and space heating—cannot be brought into the commercial picture.

The problem which the industry has to face, and it is fortunately not an immediate one, is how long will the relatively easily won oil last? The answer to that question depends on how many new large oilfields can be found and where they will be located. Taking the long-term view, it would appear that American exhaustion will come before that of the more recently developed areas like the Middle East. Fortunately for the North American Continent, which is such a large consumer, it is well blessed with alternative possibilities for sources of energy. In any event, taking North America as a whole, the story of oil reserves is still in the proving stage, and requires considerable correction in an upward direction.

Looking at the world problem, the main volume of the vast oil reserves yet to be discovered, would appear to lie outside the U.S.A. The basis of modern oil discovery today is strictly scientific. It is as well that this should be so, for applied science results in economy of effort—and economy will play an even more important part in the future, in view of the distance from oil reserves to the main centres of consumption. The large sums of money expended in the past on wild-cat drilling cannot be made available for the development of these areas.

I do not propose to discuss in detail those reserves which have been proved, as they can be assessed on an engineering basis, but I would mention that geologists estimate the world total, as now known to us, to be of the order of 16,000 million tons. This figure, which possibly errs on the conservative side, is about twenty-five times the present annual output, and represents an adequate working stock. An examination of the information on which it is based, however, reveals such great variations in the capacities of the various fields that it emphasizes the need for caution in estimating the extent of unproved reserves.

I have already referred to the continental shelf, with its large potential source of oil awaiting future development. The opinion has been expressed that some two-thirds of the marine sedimentary rocks on the earth's crust are located in the submerged seaward extension of the continents and that in them are entombed the greater proportions of the residues of the organic wastes, which constitute the source of petroleum. The continental shelf, about which much will be heard in the second half of the twentieth century, covers the area which extends, from the coastline under the sea to about the 100-fathom line. In most areas, the sea bed rapidly deepens at this point, leaving behind a platform, mainly composed of sedimentary rocks.

The search for oil in the shallow waters of the continental shelf has been in progress for some years. Special techniques for operating under water have been developed not only for geophysical surveys but also for the drilling of bore holes and the production of oil. Costs are considerably higher than on land, and as much as £1 million may be spent on a single exploratory well. It is obvious, therefore, that for economic reasons this type of exploration will require the best scientific aids and the most advanced technique.

Drilling in shallow waters adjacent to the recognized oil provinces may be expected to provide more successes than failures, but no more than a very general idea of economic reserves can be given, at this early stage, for the areas lying under deeper waters. Opinions have already been expressed that large

economic recoverable reserves, equal to double or even more than that of land oil, may be found in the continental shelf, but it must be appreciated that operations in the deeper ocean areas will demand an entirely new engineering approach. The petroleum industry has never yet failed to meet its new problems, and I am confident that it will not fail in this case.

To those studying the problem of future reserves, it is obvious that more and more research into the fundamentals of sedimentation is required, for the success of future developments on the continental shelf calls not only for a spirit of adventure but also an unusual combination of scientific knowledge and technical achievement.

In this connexion I would like to refer to the work of the Special Research Committee of the American Petroleum Institute, which is now engaged on a study of the occurrence of petroleum. This Committee is under the chairmanship of the Director of Research, Dr Robert E. Wilson, who was the previous Cadman Medallist. Their immediate programme covers sedimentology in the Gulf of Mexico, which is a very large research project dealing with fundamental principles. In the preliminary stages the study is directed towards obtaining knowledge of present-day sedimentation, which amounts to about three feet of deposits per century. The results obtained should give a better understanding of the earlier geological sedimentations, and will be of use, both in the search for new oilfields and the better appraisal of oil reserves.

This research project is designed to throw new light on the mechanics of deposition and, to this end, it has been necessary to modify methods already developed in oceanographic institutions and to adopt an entirely new technique. This involves sampling on a very extensive scale, and as a result new names have been brought into use, such as piston corer, trident corer, and others.

I suggest that some form of combined research, on these lines, should be undertaken by geologists in Great Britain. Although their work has been mainly on ocean bed studies, British geologists individually have collected a lot of data, which might prove to be of great value if collated for the common good.

Despite the satisfactory picture of future oil reserves, the petroleum industry—never complacent where scientific development is concerned—must continue to stimulate research and investigation to improve the technique of finding additional sources of the raw material. It seems to me that the considerable research carried out in this direction is not fully appreciated by those who receive most benefit from it. I refer to the peoples in those countries from which large quantities of oil are produced and exported, who, I feel, should be more fully informed of the scientific progress which is being made, more particularly in connexion with the conservation and development of the oil reserves themselves. Rarely does one see appropriate references to the benefits which accrue from the heavy expenditure borne by the petroleum industry, in providing a scientific background for present-day practice in oil developments.

The petroleum industry depends, for its raw material, on three major technical operations, each of which has its own specialized technique:

(1) the locating of oil reservoirs;
(2) the drilling of discovery and subsequent development wells;
(3) the production of the oil reservoir.

All these operations are inter-related, each playing an important part in providing adequate supplies for the future.

I make no attempt to discuss in detail the methods used in finding new oilfields, as that would be a subject in itself, but I feel I can refer briefly to the more recent developments in the geophysical approach, which plays so important a part in ensuring that adequate reserves are found—and found in good time.

Geophysical surveys, involving the gravity, magnetic, and seismic methods, were undertaken in an elementary fashion in Europe and the Middle East, before being introduced into the U.S.A., in the 1920s. American development since then has been phenomenal, both in technique and in the number of parties operating. In this latter connexion, it is estimated that at the present time they employ over 60 per cent of the world's geophysical effort.

British geophysicists pioneered in the seismic field with large scale refraction methods, primarily because these lend themselves more readily to the rapid delineation of large structures, such as are found in the Middle East. Moreover, this method is suitable in regions of poor energy returns—an unfortunate characteristic of parts of this area.

American oil company research laboratories pioneered and developed a variety of instruments designed for rapid coverage, a vital requirement in oil prospecting. Parallel competitive research in the laboratories of the larger British, Dutch, and French oil concerns, and of the Service Companies in these countries, has contributed to this progress. Large sums are spent annually on such work, and the results are made available for general application by the oil industry.

Spectacular developments have taken place in the field of under-water geophysical surveys, using gravity instruments. In the air, there has been similar progress by the use of a suitably modified form of the wartime flying magnetometer. It is not known as yet whether in structure study the use of radar and associated electronic developments will prove to be as successful as they have been in other spheres. Radar is now being used in marine geophysical survey, when working beyond the sight of land, while, in the laboratory, the electron-microscope, the mass spectrograph, and X-rays are in general use in the study of rocks samples.

I will draw only brief attention to the advances which have been made in drilling techniques, which

have made it possible for oil to be won from sources which had hitherto been regarded as beyond reach. As a result of metallurgical developments, drilling depths of 15,000 feet or more are regarded as common practice today. The world's deepest producing well, in Louisiana, was completed at a depth of 17,306 feet and took only ninety days to complete—a world record and a truly amazing performance. The greatest depth so far penetrated with the bit is in California, where drilling is now in progress in a well at more than 4 miles from the surface.

I know we can safely leave this side of the industry to meet the demands which will be made on it, and I should now like to pass on to the exploitation of the oil reservoirs.

The combining of the two words " oil " and " field " into the single term " oilfield " results in an incongruity which is not always appreciated. One's mental picture of a field is a tract of land used for pasture or for growing grain. There seems little connexion between such a scene of cultivation and the scorched sands or expanses of tropical jungle, which are the natural settings for so many of the world's large oilfields. One has to realize, however, that whereas the riches of the farmers' fields are in the surface soil, the wealth of the oilman's fields lies many thousands of feet lower down, covered by the accumulated overburden of geological ages.

In the same way as the modern farmer must study his land, in order to ensure that it is not prematurely depleted of surface vegetation or of mineral salts, so the oil producer must scientifically study all the information he can obtain on the conditions existing in his underground fields, for each reservoir differs in its many physical characteristics.

The science which has grown up in this search for information and the analysis of the data obtained has been loosely termed " petroleum engineering," or—more specifically—" reservoir engineering." The reservoir engineer comes on to the scene after the geophysicists and geologists have agreed on a favourable location, the drilling engineers have taken their borings down to the right depth, and the production engineers have completed the necessary installations to handle the crude oil and gas.

It is then the reservoir engineer's function to follow the changes in the underground formations resulting from the removal of the oil and gas. He is concerned to assess the effect of such changes on the formation fluids and to trace and predict, as far as possible, the slow large-scale vertical and lateral movement of the formation water, which usually lies under the accumulations of oil and gas. Following upon his studies, he will be able to recommend the spacing of wells and the rates and methods of production, to ensure the most efficient use of the energy stored in the reservoir, as well as the ultimate maximum recovery of hydrocarbons.

As mentioned before, Cadman's early approach to these problems had a background of conservation and orderly development. In his address to the Royal Society of Arts in 1920 he stated : " The resource and experience of the geologist are necessary to indicate where these deep-seated reservoirs existed, and the skill of the engineer is brought into play in tapping these structures with the drill in such a manner that the pent up energy is usefully employed in forcing the oil to the surface and in securing the oil deposits from the influx of water."

Whilst the geologists are more concerned with the undeveloped reserves of oil, the major oil reservoirs found by them are being developed under the experienced control of reservoir engineers. Relatively few people are aware of the extent of the research and scientific thought given to the development of these major oil deposits.

During the past twenty years, nothing has added so much to the possible reserves of oil as the empirical science of reservoir engineering, which has now reached a stage when the basic knowledge can be employed with such confidence, in planning reservoir development, that the productivity of oilfields has been increased from 20 per cent recovery to as high as 80 per cent, in favourable conditions.

The growth of this new science is, in its way, no less spectacular than the more widely publicized chemical developments in recent years. The orientation of the physicist's mind from the research laboratory to the scientific control of fluid systems, perhaps 100 cubic miles in size, demands considerable mental adaptability.

Muskat, an authority on reservoir engineering in the U.S.A., succinctly states that " oil reservoirs are controlled by a trinity of forces, hydro-dynamic stress gradient, gravity, and interfacial, superimposed on a trinity of phases—oil, gas, and water." It is an absorbing problem of great magnitude for the physicist, both in the laboratory and in the field.

The behaviour of the hydrocarbon liquids in their flow through the reservoir rocks, the knowledge of the porosity and permeability of the rocks, the fluid saturation and the chemical composition of the formation waters, and the viscosity of the fluids themselves—all must be investigated.

Oil reservoir fluid systems can be characterized by three main types, though Nature, in her fickle way, usually provides us with a highly complicated combination. The three groups are :

(1) solution gas drive—by which is meant the expansion of the gas coming out of solution from the oil, where there is no free gas above the oil and no water drive from below ;

(2) gas cap expansion drive—where the free gas cap is present ;

(3) water drive—resulting from the displacement of the oil by the upward or lateral influx of water from the large surrounding aquafer.

Of these, the water drive is the most effective.

Lord Cadman, in his address to the American Institute of Mining Engineers in New York in the early part of 1933, spoke on the subject of the scientific

control of reservoirs. He stated then that the pressure in the gas dome of the main Persian oilfield (the first to be scientifically developed as a unit) had dropped only 4 p.s.i. during the preceding four years, although a production amounting to many millions of tons had been taken from the reservoir. He attributed this to the policy of exercising scientific control which had been pursued in Persia for many years, and which has laid a valuable technical foundation for future development.

The size, the type, and the relative simplicity of the structures with which British petroleum engineers have had to deal in early Middle East developments have influenced the way they think today. Their outlook stems from the fact that from the first, they were able to produce large oilfields as units, with practically no sand problems to overcome and no question of multiple reservoirs. The result has been that observations have been made to see how the field as a whole reacted to production conditions, rather than to determine the particular behaviour of a single or a group of wells. The reservoir engineering outlook, as pioneered by the British in the Middle East, has been built up on the experience that these very large structures can each be treated as a physical entity, for most purposes. This has developed a confidence in the ability to predict and control the behaviour of large oilfields.

It is interesting to examine the scientific thought which underlies this type of approach. The concept of an oilfield which will be of uniform behaviour throughout its whole extent is uncommon, but here it was possible to measure with exactness the water levels at various parts of the field and to observe their movements and their similarity with gas–oil levels, when these appeared. It was found early on that a reasonable degree of regularity could be observed in these levels and that they could be made to respond to varying rates of withdrawal from different parts of the field.

In order to establish the levels, with a sufficient degree of confidence, and to trace their movements, it became essential to achieve greater accuracy in measuring pressures at the bottom of the well than would be required if the consideration was merely that of the draw-down pressure. The accurate recording of pressures, when used to calculate differences in the fluid levels, provides a good indication of the general behaviour inside the reservoir in a large limestone structure of this type.

To know that levels behave in a regular manner is one thing; to know the means by which Nature has arranged that they should do so is another. In the case of these limestone fields, where the body of the rock is known to be generally impermeable (perhaps less than 1 millidarcy) and therefore not capable of transporting water or oil through it with ease, the establishment of mechanism of flow inside the reservoir is of the greatest importance. British practice in these large limestone fields pioneered the concept of a well connected fissured system, which can be treated by the physicist as being uniform over the whole structure. Observations on the field, and calculations in the research laboratory, have enabled the size of the fissures to be determined and also the frequency of their occurrence throughout the structure, with the result that a clear picture has been obtained.

Furthermore, the productivity of individual wells, which is often very large, is understood in the light of these measurements, and confidence can be felt that no harm is being done by allowing high rates of flow.

The result of this approach is that, once a field can be recognized as of a uniform fissured type, it is possible to establish the necessary observations to be taken during the day-to-day operation of it, and to predict with confidence the forward behaviour of the various wells.

The major part of the oil produced in the world (over 80 per cent) comes not from limestone reservoirs of Middle East type, but from sands and sandstones, usually one above the other which may, or may not, have pressured connexion. This provides a far more difficult problem, and the way of approaching it has been pioneered in the U.S.A.

A sandfield can be regarded as a volume of fluid which contains a certain amount of energy, due to the pressure under which it exists. The problem which the reservoir engineer is set is to determine its amount, and to discover what additional source of energy of compression will be available from the large amount of water underlying the field, and how the fluids as a whole will react to a reduction of pressure.

This concept was influenced, to some extent, by the accurate measurements of the physical nature of the crude oil as it is in the reservoir, which was an early feature in the scientific study of the Middle East fields.

The knowledge which has been gained by the study of large structures has been a very valuable adjunct to the planning of new fields as energy units. In a large sandfield such as the main Kuwait oilfield, with its good porosity and permeability characteristics, large demands, consistent with reservoir knowledge, have been made. In this case, following the early experience in the Middle East limestone fields, combined with the practice and scientific understanding of sand reservoir problems, as developed in the U.S.A., it has been possible to predict with confidence that the oil and water within the reservoir will, in general, flow in a regular, and not an arbitrary, manner. Thus the reservoir engineer has been able to predict with confidence, over many years, the exact behaviour of the average well and the movement of the reservoir fluids. In this work the co-operation of American and British reservoir engineers has resulted in a scientific " melding " of high order.

In recent years a most powerful tool has been added by American scientists to the equipment of the reservoir engineer. I refer to the electrical reservoir analyser. By means of this instrument, it is now possible to predict with a reasonable degree of accuracy what will be the future producing history of an oil

reservoir over an interval equal to that during which its past history is known. The operation of these instruments is based on the analogy which has been shown to exist between the flow of fluids through porous media and the flow of electric currents through conductive media.

The effect of differences of pressure which develop in different parts of an oil reservoir during its producing life, and which cause movements of fluid through the porous formations, can be adequately represented in the electric analyser by differences of potential which cause the flow of electric currents. The permeability of the porous formations to fluid flow becomes electric conductivity, and fluid volumes are represented by electrical capacity.

Networks and circuits, containing capacities and resistances, can therefore be set up, which will respond to changes in a similar manner as the oil reservoir reacts to changes brought about by the production of oil and gas.

There is, moreover, the significant advantage that the results of any change impressed on the electrical circuits, set up in the reservoir analyser, can be determined in a fraction of a second, whereas the corresponding period of change in the oil reservoir itself might be a matter of years. This speeding up of the performance time enables a "trial and error" method to be adopted so that, over a given period of known past history, the behaviour of the reservoir as set up in the electrical analyser can be compared with the actual performance obtained from the oilfield itself.

When once this "match" has been obtained, the predictions of the instrument, as regards the response of the reservoir to future rates and methods of production, can be accepted with confidence.

It is, of course, obvious that the answers given by the electrical analyser to various problems of oilfield production and operation cannot be more accurate than the data supplied to it. The instrument is, to a certain extent, self-checking, and it is possible, if required, to adjust the basic data in order that the results may correspond with the known producing history of the field.

Continual experiment and research are being undertaken to develop measuring instruments of the greatest sensitivity and precision, for the progress of the science of reservoir engineering, in common with other sciences, depends on accuracy of measurement.

I have already indicated in general terms the measurements and tests required in connexion with reservoir engineering, but I should like now to refer to them in greater detail.

They include the following:

(1) the pressure and temperature existing throughout the producing formations;

(2) the permeability and porosity of the reservoir rocks;

(3) the percentage saturation of the formation with gas, oil, and water, and the irreducible minimum of the water content under reservoir conditions;

(4) the compressibility under reservoir pressure of the various formation fluids;

(5) the determination of the "formation volume factor," which is the volume occupied by one barrel of stock tank crude when it is in its original position in the reservoir and carrying its associated gas in solution;

(6) the obtaining of bottom hole samples of reservoir crude and the determination of the viscosity, specific gravity, saturation pressure, and gas solubility of such samples under artificially-reproduced reservoir conditions;

(7) the rate of invasion of the oil-bearing rocks by the underlying formation waters.

It is not possible to review here all the instruments developed by the various oil companies. A typical example, however, is one developed by the Gulf Oil Corporation used for measuring formation pressures which, when lowered into the well, makes possible the measurement of the static and flowing pressures at a point opposite the producing formation. The difference between these two pressures is known as the "producing draw-down," and this gives a measure of the well's productive capacity. The production in barrels divided by the draw-down in lb per square inch, is known as the "productivity index" of the well.

The accurate determination of the fixed characteristics of the reservoir, permeability, porosity, oil, and connate water content of the reservoir rocks, calls for the taking and examining of a large number of core samples. Not only must these samples be carefully cut and selected, in order to be truly representative of the stratified formations, but they must also be most carefully and expeditiously handled, once they have been taken from the core barrel. To ensure that the water in the normal drilling mud may not "invade" the specimen cores, special "low-water-loss" muds have been developed, and the use of oil-base mud is becoming widespread. When it is intended to ship the cores to a distant laboratory, they should be dipped into liquid paraffin-wax immediately on taking from the core barrel and then sealed for transport.

The examination of bottom-hole samples of reservoir fluids is made in specially designed pressure–volume–temperature apparatus, in which the original conditions of pressure and temperature can be reproduced. The "bubble point" of the oil in the reservoir is a most important characteristic, as it determines the pressure at which free gas will be liberated.

At any given depth the formation pressure of an originally under-saturated crude declines with time and, in so doing, approaches the saturation curve. Pressure decline beyond this point results in the liberation of free gas in the pore spaces of the reservoir rocks and a waste of reservoir energy.

The useful producing life of a well can be curtailed by contamination of the crude with formation water,

which usually has a high salt content. The reservoir engineer is concerned, therefore, at all times, to know in which direction and at what rate the invading waters are approaching the well bores. This is not easy to ascertain, but the subject is of such importance that certain wells are selected for observation purposes only. In these wells, which are not produced, the oil column is maintained in a steady, conditioned, gas-free state, and the depth of the oil–water contact in the well bore is periodically measured.

A recent approach to this problem is to line the bore of the observation well with a plastic, non-metallic casing, and to detect the rise of water through the formation by an electrical logging device, in which circulating electric currents are induced in the surrounding formations without the necessity of direct contact by current-carrying electrodes.

It is in view of this great variety of physical problems that large oil-producing companies, which have available the resources of highly trained technical staff, consider it desirable to appoint special study teams of experts in reservoir engineering. Such a team has no concern with matters of the day-to-day operation of the field, but is responsible for pointing out trends in reservoir behaviour resulting from current rates and methods of production and for making recommendations on future policies, designed to secure the most efficient production over the longest time.

Parallel with the applied research in the oil company laboratories, fundamental research is in progress in the U.S.A. I should like to refer to an important study now in progress by an American research group, under the chairmanship of Dr Muskat, on the mechanism of the displacement of oil from porous materials. A knowledge of the multi-phase flow through porous media is an important key to the problem of producing the maximum oil from any reservoir.

When all has been said, however, and the most scientific devices have been brought into use, it will be appreciated that this new science of reservoir engineering calls not only for a high degree of technical skill in experimentation, but also for a lively imagination in order to appreciate the changes which are taking place in these vast reservoirs, perhaps 100 cubic miles or more in size, and many thousands of feet below the surface.

Very great effort is being devoted to the reservoir mechanics of oil recovery, and this, together with the confidence which large-scale development based on years of research and practice has provided, is a great scientific contribution which the petroleum industry is making to the oil-rich countries of today.

The subject I have dealt with is a vast one and, in order to complete a picture, it has been necessary to treat superficially matters deserving a more thorough review. The conclusion can be drawn, however, that scientific and technical developments, both in finding and winning oil, will ensure that adequate supplies will be available for a long time to come, but the free flowing distribution from the major regions of supply to all the countries where they are needed in the service of man, must also be assured.

I have drawn particular attention to the development of reservoir engineering as being a great scientific and technical contribution, which is providing a major benefit to those countries blessed with oil resources, by ensuring that such resources are developed in their best interests.

I have referred to American and British co-operation, and I wish to pay a tribute to those American scientists, dealing with oil problems, who so freely make available the results of their studies and experimentation for the common benefit.

In conclusion, I should like humbly to express my appreciation of the privilege afforded me of standing at this historic table, which is a unique experience and to quote from a contribution to the *Philosophical Magazine* by Michael Faraday in 1844:

> "It is always safe . . . to distinguish as much as is in our power, fact from theory; the experience of past ages is sufficient to show us the wisdom of such a course; and considering the constant tendency of the mind to rest on an assumption, and when it answers every present purpose, to forget that it is an assumption, we ought to remember that it, in such cases, becomes a prejudice, and inevitably interferes, more or less, with a clear-sighted judgment. I cannot doubt but that he who, as a wise philosopher, has most power of penetrating the secrets of nature, and guessing by hypothesis . . . will also be most careful for his own safe progress and that of others, to distinguish that knowledge which consists of assumption, by which I mean theory and hypothesis, from that which is the knowledge of facts and laws; never raising the former to the dignity or authority of the latter, nor confusing the latter more than is inevitable with the former."

The petroleum industry cannot afford to place too much reliance on hypothesis, and prides itself on obtaining the scientific facts in all its operations. I hope that I have demonstrated this in my discussion on the problems connected with the winning of oil.

PRESENTATION OF THE CADMAN MEDAL

Lt.-Col. S. J. M. Auld, O.B.E., M.C., president of the Institute of Petroleum, who occupied the chair at the conclusion of the lecture, said:

My Lord, Ladies, and Gentlemen. The pleasure of listening to this lecture has been added to, I am sure for all of us, by reason of its having closely associated once more two men whom we wish to praise and honour, and who, in the past, were personally so intimately connected—the late John Cadman and Philip Southwell.

The Cadman Memorial Lecture is delivered by the man to whom the Cadman Medal has been awarded. On behalf of its founders the award is in the power of the President and Council of the Institute of Petroleum, and is made at intervals to men of eminence who distinguish themselves in the application of science in the domain of petroleum, irrespective of their nationality or membership of the Institute of Petroleum.

The recommendation by the Awards Committee of the Institute that Mr C. A. P. Southwell be invited to receive the Medal and to give the Fourth Cadman Lecture was unanimous. So also was the decision of my immediate predecessor as President of the Institute of Petroleum and his Council. A better or more suitable choice could not have been made. Not only has Mr Southwell distinguished himself in the sphere of petroleum, but he has done so in the true Cadman tradition, and has thereby brought honour not only on himself but also on the Institute of Petroleum, on the British petroleum industry, and to his country.

I do not propose epitomizing Mr Southwell's petroleum career as geologist and mining engineer in those sort of places in the world where oil is found. I will not dwell upon his successive successes in the field, nor his ability as a man of science to deal with and handle the toughest factors in oilfield and oil production development.

I would like rather to refer to his most recent and more spectacular achievements of which today he has given us a glint. Here is a man who never sought the limelight on the stage of life but who, when it did fall upon him, could be depended upon to be seen, as Ecclesiastes enjoined—"doing with his might that which his hand found to do"—and doing it as I have said before, "with his device, his knowledge, and his wisdom."

MR C. A. P. SOUTHWELL RECEIVES THE CADMAN MEDAL FROM THE PRESIDENT

Mr Southwell was able, by near heroic efforts during the war, to develop our tiny English oilfield at Eakring to a stage where its output gave us a key proportion of the highly specialized oils essential to the R.A.F. Hardly without breathing space, he was to be found, with the same absence of show, developing a field with a present production 400 times that of Eakring; done, moreover, under conditions of stress, and shortage of material, and political difficulty which emphasize the credit due. Due, may I put it, to the wisdom of the successive Kuwait Rulers, the joint British–American Company operating, and Philip Southwell.

Philip Southwell, you have done much to deserve our thanks for your perpetuation of the name and tradition of John Cadman, first Lord of Silverdale, the famous leader whom we are here to praise. It gives me the greatest pleasure to present you with the Cadman Medal.

The Cadman Memorial Medal was then handed to Mr C. A. P. Southwell amid prolonged applause.

THE INSTITUTE OF PETROLEUM

An Ordinary General Meeting of the Institute of Petroleum was held at 26 Portland Place, London, W.1, on 9 February 1955, the Chair being taken by Professor F. Morton, a vice-president of the Institute.

Prof. F. Morton: I wish to convey to you the President's apologies for his absence this evening, and I am sure that you would like the Secretary to send to him the meeting's sympathy on his recent bereavement.

The Editor then read the minutes of the previous meeting, which were confirmed and signed as a correct record. He also announced the names of members elected since the previous meeting, and gave notice of the date of the next meeting.

The Chairman then introduced Dr C. G. Williams, who presented the following paper:

APPLICATIONAL RESEARCH IN THE OIL INDUSTRY

By C. G. WILLIAMS* (Fellow)

The oil industry invests heavily in research, the bulk of it being carried out in the U.S.A., where 60 per cent of the tonnage of the world's oil products are sold. The estimated expenditure in the U.S.A. was $100 million p.a. in 1951, while a probable figure for the U.K. is £2 million p.a. Such figures, while large, are not unduly so in relation to the magnitude of the oil industry. It is customary to express research expenditure as a percentage of the gross turnover of a company or industry, and the figure for the U.S. oil industry is approximately 0·6 per cent. For comparison, a typical figure for the chemical industry lies between 2 and 3 per cent, and there are other industries which spend even more in relation to their sales, though in such cases there is often a strong element of Government support. There are other industries, however, which spend less. The O.E.E.C. has recently published† figures of "national civilian research budgets" in which research expenditure is expressed as a percentage of the gross national products. The figure for both the U.S.A. and the U.K. is 0·5 per cent—so that the petroleum industry is close to the average.

Such percentage figures are the net result of a large number of factors such as:

(1) The scope or need for research, *i.e.* whether it is possible, practicable, or desirable, to make improvements in a given field. For example, the scope is likely to be greater for motor gasoline than for fuel oil, and greater for chemicals than for gasoline. An important factor here is the "newness" of a field, and whether the market is an expanding one. Again, questions of the profitability of the product are obviously relevant.

(2) The number of research ideas: the larger the number, the greater the possible expenditure on simultaneous or consecutive lines of investigation. On the other hand, while the need may be great, the number of ideas may be all too small.

(3) The availability of finance, both operating and capital, for expenditure on laboratories.

(4) Similarly, the availability of finance for implementing the results of research—in terms of plant and equipment, operating costs, marketing costs, etc. For example, it is possible to envisage projects arising from research having to be shelved because of insufficient capital. It has been stated that the ratio of expenditures on research, development, and manufacturing plant for a given project is 1 : 10 : 100.

(5) The availability of scientific manpower suitable for research and for implementing the results of research. This has become a very important limiting factor due to present supply not meeting the demand. A correct balance has to be maintained between manpower used for research itself and manpower required to ensure full use of research results.

(6) The general competitive situation.

(7) The amount invested in research and development by other industries which may tend to set the pace for the oil industry, *e.g.* the automobile industry. The development of new equipment or appliances may require new oil products, *e.g.* special fuels and lubricants for jet aircraft. Failure of an engineering industry to tackle its own developments adequately may also put an additional research load on the oil industry, which has then to develop products which are unnecessarily "special."

(8) The "expensiveness" of a given field of research. Certain kinds of research are relatively cheap, but where costly engine tests, pilot plant studies, or special analytical techniques are required, the expense is correspondingly inflated.

Each of the above factors should be weighed up in considering the intensity of effort to be devoted to a particular field of research. Failure to do so may result in misguided or wasted effort.

* "Shell" Research Ltd.

† O.E.E.C. Report on the Organization of Applied Research.

PURE, APPLIED, AND APPLICATIONAL RESEARCH

Research can be defined as the use of scientific methods for making new discoveries and a distinction is usually drawn between "pure" research and "applied" research. In general, most pure research is carried out at universities, and is concerned with knowledge for its own sake. The kind of research carried out by the oil industry is mainly "applied," *i.e.* directed, towards some known objective. It is, however, important to recognize the indebtedness of industrial or applied research to pure research. Applied research is, to a certain extent, carried out in "commercial blinkers," and pure research, with no such blinkers, can frequently suggest an entirely new route. It can also help in providing new techniques of analysis, etc., which can become very important tools in applied research. Finally, industrial research is dependent on a supply of scientists trained, to a large extent, in post-graduate schools devoted to pure research in British universities.

Accepting, therefore, that applied research is the main category of research carried out by the oil industry (as well as by other industries), it is perhaps time to explain what is meant by "applicational" research.

The operations of an oil company can, for convenience, be divided under the following headings:

(*a*) Exploration and production.
(*b*) Manufacture.
(*c*) Transportation and storage.
(*d*) Marketing.

There are, corresponding to the above, similar divisions possible in regard to the research effort.

Exploration research embraces the geophysical and geological studies concerned with the discovery of oil, while production research is concerned with the problems of drilling and of bringing oil to the surface as effectively and as cheaply as possible.

Corresponding to manufacture, there is "process research," in which one is attempting to develop new processes or improvements in existing processes which will give, for example, a greater yield of useful products, reduced manufacturing costs, etc. In addition, there are problems of refinery engineering and of instrumentation.

The transportation and storage of products may give rise to their own range of problems which require investigation. These may relate to problems of corrosion of equipment, and contamination and deterioration of products, etc.

Finally, corresponding to marketing, there is the field of "applicational research" which is concerned with the whole subject of product performance, and the development of products which are suitable for a given "application." Research is here closest to the user or customer and, therefore, to the sales side of the business. It is also close to the manufacturing side, since, to improve a product, some change in manufacturing procedure is usually necessary.

In general, applicational research begins and ends in the field. It begins in the field because it is there that the need arises and the performance requirements are set. It ends there because the results of successful applicational research are used in the field. Good applicational research cannot therefore begin until there is a proper appreciation of the nature of the problems in the field, and this may frequently necessitate personal contact of the laboratory research worker with the field so that he can see for himself the nature of the problems and of the service conditions, and have access to any data, statistical or otherwise, which would add to his general background knowledge. For example, if "deposits" are stated to be giving trouble in some particular application, the research worker should have the opportunity of inspecting such deposits *in situ*, he should ascertain from the operators of the equipment the precise conditions of use, and should be able to take away for analysis samples of the deposits. The research worker can also learn a lot from controlled field trials, which he himself may supervise or participate in, in which product performance is measured under actual service conditions.

PRODUCT PERFORMANCE CRITERIA

In general, product performance factors selected for study are those which are known to be important, which may have actually given trouble in service, and which appear capable of improvement. The oil industry—with the exception of the rapidly developing field of chemicals from petroleum—is predominantly concerned with the production of energy, and it is possible to express, in general terms, the performance criteria which dominate most applicational studies. They are: stability; deposits; durability; starting-up; power; and efficiency. These criteria determine, to a large extent, "customer satisfaction," and the following brief notes explain what is meant by them:

Stability. A product, while being stored or transported, should not undergo changes which would affect the performance in the final appliance. Such changes may result from oxidation, polymerization, and other chemical and physical effects which are, therefore, often involved in applicational research.

Deposits. Deposits of one kind or another constitute one of the main performance factors of fuels and lubricants. Deposits of carbon and of lead compounds in the combustion chamber of petrol engines, deposits on injector nozzles of diesel engines and on atomizers of gas turbines, deposits in piston ring grooves, on piston surfaces, on crankcase walls, in oil sumps, in oil-ways, in carburettors, inlet pipes, on inlet valves and exhaust valves—such deposits, if they are excessive or of the wrong kind, can become limiting factors in the performance of an engine. In boiler

plant, deposits may also cause corrosion troubles and interfere with heat transfer and, in the gas turbine, they may cause difficulties with turbine blades. An understanding of how such deposits are formed, the development of analytical techniques for their examination, and product development to minimize their occurrence are, therefore, an important aspect of applicational research.

Durability. An extended useful life and comparative freedom from the need to effect repairs of an appliance is another criterion of satisfactory product performance. This, in some respects, may be related to the preceding paragraph on "deposits," some of which affect durability. Durability may also include problems of wear and of breakdown of lubrication, *e.g.* wear of cylinder and piston rings, of bearings and crankshaft, while the transmission may introduce problems not only of wear, but also of breakdown of lubrication resulting in such phenomena as scuffing.

Starting-up. A product may frequently be judged by the ease or otherwise with which the appliance or power unit can be got under way. In this fashion are introduced problems of fuel volatility, ignition quality, lubricant viscosity, etc.

Power. The main contribution which applicational research can make in this field is to develop fuels and lubricants which permit maximum power, if necessary, for prolonged periods. The route which can be followed obviously varies with the application—in the diesel engine it is probable that the lubricant rather than the fuel is the limiting factor, while in the petrol engine anti-knock quality is of primary importance. There is, in addition, a field of research which is concerned with reducing power losses; such losses may result from deposits—mentioned earlier—which cause plug fouling, pre-ignition, etc., or losses which may be reduced by lowering engine friction.

Efficiency. Under this heading falls not only the cost of the product to the customer, but also its cost to the manufacturer. It is obviously desirable, in both cases, that the product should be as cheap as possible, and, in addition, that its utilization should be efficient. For example, anti-knock quality of gasoline affects the attainable efficiency of utilization. Similarly, any reduction in losses, such as those mentioned in the previous paragraph, helps to improve the efficiency of utilization.

In parenthesis, it may be commented that it is truly remarkable how much product development depends these days on the use of additives. Their number is legion and their mode of action extremely varied and versatile. The use of small quantities of active chemicals is often the most effective and cheapest method of improving product performance, whether it be an anti-knock agent such as TEL, an anti-oxidant to reduce bearing corrosion, a phosphorous additive for altering ignition temperatures and electrical conductivity of combustion chamber deposits, a silicone for reducing foaming, a calcium salt for neutralizing corrosive combustion products, and so on. The additive approach is not, however, as simple as it may seem; for example, undesirable side effects have always to be guarded against.

The above brief discussion of performance problems and requirements is given merely to illustrate the wide scope of applicational research. There are many other illustrations that could be given; for example, there are many problems concerned with special requirements, *e.g.* in the aviation field where extremes of temperature—both for fuels and lubricants—are involved. Enough has been said, however, to emphasize that, in applicational research, it is necessary to have in mind the particular performance criteria which are to be used in judging a product and, ultimately, in judging whether success has been achieved.

OTHER OBJECTIVES OF APPLICATIONAL RESEARCH

It is necessary to point out that research is sometimes carried out with the object, not of improving or cheapening a product, but of maintaining existing product quality. The oil industry is based on a raw material which varies in different parts of the world, and, even from the same well, crude oil may change considerably in composition during the lifetime of the well. It is, therefore, necessary to watch product performance very carefully to ensure that changes in crude do not result in changes in product quality. This may, of course, involve studies of process variables which are outside the scope of this paper, but product performance is the final criterion.

Not only are there changes in crude which may affect product performance, but also changes in refinery scheduling. A refinery is a very flexible manufacturing machine which produces a very wide range of products. The quantity and properties of one product may have to be varied for manufacturing or marketing reasons, thereby, possibly, affecting the properties of the other products. It might be assumed that such variations can be taken care of by refinery control methods, and this is true up to a point. Refinery test methods, in general, control certain physical and chemical properties and do not usually measure performance factors. One of these days we may know so much about the relation between the physical characteristics and chemical constitutions of oil products and their performance that an adequate repertory of test methods may be possible. For the time being, applicational research techniques, such as engine tests, are frequently needed to ensure the maintenance of product quality.

Another objective of applicational research is what might be described as the better use of products. On the assumption that customer satisfaction is one of the main objectives of applicational research, it frequently happens that the customer may blame the oil product, even though the deficiency is the fault of the appliance which he is manufacturing or operating. This can be expressed in a different fashion by saying that if customer satisfaction can be

increased by better performance, then the market for the oil product may be extended, whether that improvement has been effected through the oil product, or through the appliance, or by a combination of both.

This general philosophy has been expressed previously * in the form of a simple qualitative equation, as follows :

Quality of mechanical design	×	Quality of fuel	×	Quality of lubricant	=	Quality of performance

From the above equation it is obvious that the quality of performance of the appliance is highest when both the quality of mechanical design and of fuel and lubricant are all as high as possible. If, however, one of the factors on the left-hand side is deficient, then it may be possible (within limits) to make up for its deficiencies by ensuring that the other factors are compensatingly high. If, for example, the quality of mechanical design is deficient, then the oil industry may be called upon to supply a fuel or lubricant with special qualities in order to give a final overall result which is tolerable.

In general, it is the responsibility of the manufacturer of the appliance to ensure an adequate standard of mechanical design. For example, the ability of a motor car engine to start under cold conditions can be improved by attending to various features of design, such as the size of the battery, the design of the starting motor, the type of carburettor, and so on. On the other hand, starting can also be improved by adequate volatility of the fuel and by using a lubricant of sufficiently low viscosity at starting temperatures. It is thus possible to introduce into the above equation the appropriate factors as follows :

Quality (size) of starting equipment	×	Quality of carburettor design (mixture richness)	×	Quality of fuel (volatility)	×	Quality of lubricant (low viscosity)	=	Quality of starting

If the starting equipment is inadequate in size or condition, or if the carburettor does not produce the requisite starting mixture, then it may be necessary to improve the quality of the fuel out of all proportion (in this case quality being defined by volatility) or to use a lubricant of exceptionally low viscosity. In this particular instance, the mechanical design factors involved are clearly the responsibility of the manufacturer of the component involved, and not that of the oil industry.

Another, and perhaps simpler, example of the above equation concerns bearing performance as determined by corrosion. Thus :

Corrosion resistance of bearing material	×	Oxidation resistance of lubricant	=	Degree of freedom from bearing corrosion

This is a little nearer to the sort of problem the oil industry may study in its applicational research laboratories. Certain bearing materials are undoubtedly prone to corrode, *e.g.* copper–lead, but by certain treatment, or by the use of alternative materials, *e.g.* aluminium–tin, liability to corrode is considerably reduced. Quite frequently, the applicational research laboratory, in its study of product performance in actual appliances, learns a good deal about these appliances and their deficiencies. Such information may be of great help to the designer of the equipment. Occasionally, the applicational laboratory is tempted to delve more deeply than perhaps is justified into such mechanical and metallurgical problems.

A related type of applicational research is concerned with developing or encouraging a market for a new product or for a product which is "long." A new product happens rather infrequently these days, except in the field of chemical products made from petroleum. Uses for all the various products which are derived from a barrel of crude have been developed, ranging from liquid petroleum gas at the top, to bitumen at the bottom. There may, however, be circumstances leading to a surplus, or potential surplus, of one particular component or fraction, and an applicational laboratory may be called upon to help to develop or extend the market by developing uses. This has been done, in the past, for middle distillates, wax, and bitumen.

Finally, it should always be an important objective of applicational—as of other kinds of research—to increase "background" knowledge even though it may not appear to have any immediate "application." The most satisfactory solution to a problem—and sometimes the quickest—is achieved by a real understanding of the factors involved. A good deal of such work is published and thus serves, "To contribute to the common store of technical knowledge, with the ultimate motive of increased markets through better standards of living."†

TEST METHODS

Studies of the kind described above obviously require laboratory test techniques in which it is hoped the essential conditions determining product performance have been reproduced. This is frequently the most difficult aspect of applicational research and the one involving the largest number of pitfalls for the unwary. Ideally, before any laboratory studies are carried out, all the factors controlling product performance and their relative importance should have been determined. To do this completely is usually impracticable, mainly because there is not enough opportunity, time, or money. All the same, one very important ingredient in successful applicational research is a preliminary study of field conditions. If the objective is to develop a lubricant or a fuel which has to withstand certain extremes of temperature one should at least know what those extremes are.

* Ref. Akroyd Stuart Lecture, Nottingham University, February 1953.

† O.E.E.C. Report on the Organization of Applied Research.

If an improved test for anti-knock qualities is required, one should know what conditions of mixture strength, power output, speed, engine temperature, and so on should be simulated. If, in addition, new requirements are involved, they should be specified and their significance to the customer understood.

On the assumption that something, at least, is known of the end-use and of the conditions of usage of the product, the problem is transferred to the laboratory, which is then faced with the task of developing a new or improved product. For this purpose, laboratory test methods are required, and applicational research involves, initially, the use of these test methods to develop products which show improved performance in the laboratory. At a given stage such products can be subjected to field trials to ascertain whether the desired objective has been achieved and, ultimately, manufacturing and marketing take over the new or improved product. This chain of events is illustrated in Fig 1. The pitfalls are also shown.

Fig 1

It has already been pointed out that, in translating field data into the laboratory, risks are usually taken because of the inadequacy of information, and this may lead to certain false assumptions. The first may be an over-simplification of the variables involved. Necessarily, a laboratory technique is used under controlled conditions, and certain assumptions are made regarding the essential factors which are of most importance. Here mistakes can be made through lack of knowledge. There was, for example, a time when it was assumed that the most important requirement of an engine lubricant was to resist oxidation, and various test methods were developed to measure this property. As knowledge grew, it was realized that oxidation involved certain catalytic effects, and catalysts were therefore introduced into the oxidation experiments. These catalysts became more and more complicated as more became known regarding the catalytic influences present in an engine. All this work, interesting and useful though it was, missed the essential complicating factor that the most important deteriorating influences to which a lubricant is subjected in an engine originate, not from oxidation phenomena, but from contaminating influences from the fuel and its combustion products. Laboratory techniques were therefore developed for measuring properties such as "detergency"; then there was a realization of the important part which basicity in a lubricant could play in neutralizing corrosive products originating from the fuel, and so on. All this work emphasized the misleading conclusions which could be arrived at as a result of over-simplification of test methods.

The development of the F1, F2, F3, and F4 knock test methods is a good example of the amount of effort devoted to simulating certain essential conditions occurring in different types of engines under various conditions. The F1, F2, and F3 take care of three different degrees of "engine severity," the first two in automotive engines and the third in aviation, while the F4 is intended to represent rich mixture take-off conditions in aero engines. That none of these tests is entirely satisfactory, in terms of correlation with conditions in the field, is illustrated by the continuing work relating F1 and F2 ratings to the performance of fuels in average automobiles on the road.

It is therefore very important in the development of test techniques to recognize what simplifications have been made and to be ready to abandon such simplifications when further knowledge shows this to be necessary.

Another trap into which the experimenter may fall all too readily in developing his test methods is to aggravate or accentuate certain conditions which will give him quick results. This is understandable. The normal life of the product or the appliance using the product may be very large, and the experimenter cannot afford the time or the money to study the problem under conditions exactly paralleling those occurring under actual customer conditions. He therefore introduces conditions of higher temperature, higher pressure, higher speed, and so on, which will enable him to obtain results more quickly and, in doing so, he may completely distort the picture. The study of fuel or lubricant variables in regard to exhaust valve life is a good example of this. The normal life of an exhaust valve is about 1000 hr, which would involve more than a month's continuous operation, day and night, or probably two or three years actual use. By running an engine continuously at high loads and high speed, conditions can be introduced which cause failure, say, within 100 hr, and this is obviously an attractive procedure for the impatient experimenter. Unfortunately, however, the raising of valve temperatures gives rise to a range of deposits having melting points differing from those formed at lower temperatures, and these deposits have corrosive

and other effects differing from those which are normally experienced. The experimenter may, therefore, be tempted to draw conclusions from such studies which are quite misleading when applied to the conditions operative in the average motor car engine. Not only can he be misled by operating the engine at higher temperatures, but also by employing continuous instead of the intermittent conditions experienced in normal service. The cooling down of an engine and the heating up when it is re-started are essential factors in the life of any engine and, if such factors are not introduced into a test technique, misleading conclusions may be drawn.

There is, in addition, an attitude of mind which can sometimes afflict the experimenter. It is one of over-confidence or over-absorption in the test method or apparatus, so that it becomes an end in itself rather than a means to an end. An enormous amount of ingenuity can be devoted to the development of a machine supposed to simulate some particular problem, *e.g.* that of wear or durability, so that it becomes the pride of its inventor, who looks upon it as having an intrinsic importance. The result is that more and more effort can be devoted to the machine, studying the variables involved *ad infinitum*, with complete disregard of its significance and of its correlation with service conditions.

The foregoing emphasizes the importance of continually reviewing critically the test methods which are used, and being prepared to modify or adjust them as further knowledge may require. This knowledge may come from field trials. A stage may be reached in the development of a product in the laboratory at which it is opportune to try it out in the field to ascertain whether success is round the corner. How frequently are these field trials disappointing! They are disappointing because of the non-recognition of some factor which may have been considered to be of secondary importance but which turns out to be the Achilles heel. Such failures, however, can be turned to good account. They can show in what ways laboratory techniques should be modified or extended and, if such disappointments are made use of, they can often become the turning point, not only in the development of a better product, but also in the development of a better test method.

Before leaving the subject of test methods, reference should be made to the desirability of making full use of statistical techniques in developing new test procedures. In the early stages, correlation data are usually clouded by appreciable experimental errors, both in the test and field results, but, in spite of this, statistical analysis of the data will, if the work has been properly planned, show whether the correlation is inadequate or might be made satisfactory if further work were carried out. Statistical techniques should sometimes be used from the outset in the planning and preparation of an experimental programme. In particular, given a knowledge of the magnitude of the experimental errors—which may be available from previous experiences—it is possible to predict the minimum amount of work that will be required to meet the objectives of the programme. Quite apart from their value in achieving experimental economy, statistical techniques, by their insistence on detailed planning of the work, often have the salutary effect of crystallizing the objectives.

Special reference should also be made to the increasingly important part which analytical techniques are tending to play in applicational research, so much so that it is usually desirable to set aside a certain amount of effort for the development and application of newer techniques, especially physical methods of analysis. Such techniques are powerful aids in the study of deposits of various kinds, *e.g.* those occurring on the various parts of a sparking plug, and in diagnosing troubles of various kinds. Spectroscopy and spectrometry in their various forms have become essential for constitutional studies from which an understanding of the relation between composition and performance may emerge, thereby removing much of the trial and error approach in product development.

ORGANIZATION OF APPLICATIONAL RESEARCH

Studies of the performance of products in appliances may lead quite deeply into realms of research where engineer, chemist, and physicist have to collaborate. For example, in a research on cylinder bore wear, the engineer will study the influence of mechanical design and operating variables on wear, the physicist may bring the latest radio-active tracer techniques to facilitate wear measurement, while the chemist will use his knowledge to develop lubricants and additives to combat wear. The problem is, of course, to make quite sure that all these kinds of research worker co-operate satisfactorily and take full advantage of the various techniques which each can provide. This can best be done by a flexible organization which does not divide research teams into professions, but into research objectives. If there is a particular problem on, say, the lubrication of gears, the engineer, chemist, and physicist should work side by side, each learning from the other and contributing his own special knowledge. The general philosophy behind modern organization in research is that, broadly speaking, each employee is trained to be of most use in a particular sphere and that, by co-operation with others, more and better results can be achieved more cheaply than by indiscriminate individualism.

Fig 2 shows how a large applicational research laboratory can be organized under the two main divisions of research and services. Four research teams are shown, each engaged on a particular project. They are supported by a variety of services, which include a machine shop and its associated drawing office; an instrument shop, where not only instruments can be inspected and repaired, but also developed; facilities for routine examination and analysis for chemical, physical, and metallurgical

properties. In addition, there may be a photographic section, stores, printing and publications section, library, and a section which deals with personnel problems and with the costing and budgeting of the laboratory. Roughly, for every man engaged directly on research, there is required at least another for the sort of services mentioned.

There is need for occasional compromise in this matter of centralization in service. For example, while it may be necessary to have a centralized machine shop available for the research laboratory as a whole, it is sometimes justifiable to install a small lathe or a small drilling machine, adjacent to some rather highly specialized need. This may occasionally lead to increased efficiency and to greater interest and morale generally on some particular problem. The main purpose of a research laboratory, after all, is to produce original and good work, and not to run a highly efficient organization as an end in itself.

A sufficient proportion of the entire effort of a laboratory should be devoted to a fairly free exploration of certain subjects, where both the man himself and the idea he has would appear to justify it. There are some classes of research for which it is impossible to predict the outcome and, provided the facilities of a laboratory are not consumed in a number of wild-goose chases, it is desirable to encourage a certain proportion of effort which will not be subjected to rigorous scrutiny. There are, in every large research organization, a few research workers who should be given a large degree of freedom and who will do their best work under those conditions. There are others, however, who work best on the planned type of research with a fairly well-defined objective. In every case successful results presuppose the right kind of scientist with adequate facilities at his disposal, with the best possible contacts with the field, and with plenty of help and advice from the marketing and manufacturing departments of his company.

British research efforts are sometimes criticized on the grounds that, whilst the U.K. produces more first-rate pure research for its size than any other country, it fails to apply the results of this pure research. Applicational research of the kind described in this paper is an important contribution towards answering such criticism and ensuring that the best possible products are available at the lowest possible cost.

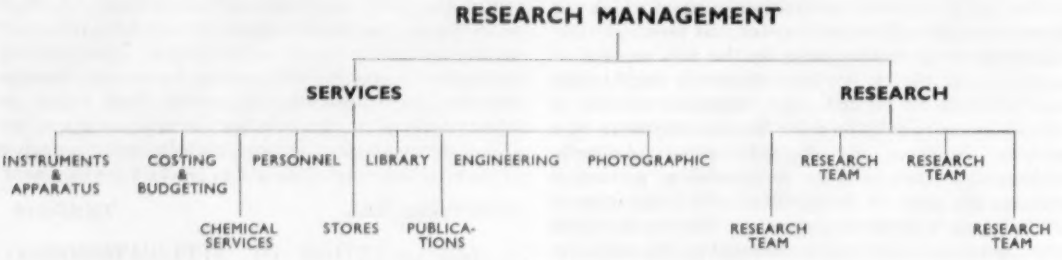

FIG 2

ORGANIZATION OF A LARGE RESEARCH LABORATORY

"The supporters of applied and developmental research are not content with the intellectual growth of the researcher. They want results—and they want them on time, before another company, industry, or country attains them. This leads to research planning, to the definition of objectives, to directed effort, and to scheduling. Thus it is that the scientist in the applied or developmental field is simultaneously lashed by necessity and stimulated by rewards. He is challenged by competition, disciplined by deadlines, judged by results—and he thrives and produces."*

DISCUSSION

C. S. Windebank: In his illuminating and very interesting paper, Dr Williams has thrown light on many of the factors regarding the operation of an industrial research laboratory, and in particular on those points which concern the quality of the products of the petroleum industry, and their performance and use in equipment. He has told us about the level of expenditure in industrial research within the industry, both world-wide and in the U.K. He has also defined the position of applicational research within an industrial organization, and sub-divided it into the more important categories concerned with the production of power. He has shown us how applicational research is handled in the development of a new product, and emphasized the many pitfalls which await the unwary experimenter. Finally, he has given us some hints on the organization of a laboratory engaged in this type of pursuit.

Dr Williams ends his paper with a striking quotation which emphasizes the essential difference between academic research and industrial research—the time factor. In an academic laboratory, all considerations can be submerged in the interest of scientific exactness. In the industrial laboratory, without giving way at all in that respect, the research man has to remember that time is the essence of social progress today. This means that we must expect in an industrial research man

* O.E.E.C. Report on the Organization of Applied Research.

all the qualities which are found in an academic research man, and something more. I make this point quite seriously, because I feel that there is an idea prevalent in the U.K. that industrial research is a sort of poor cousin to its academic counterpart. The industrial research man seldom has time to collect all the scientific data that he would like to have to help him in his project. This means that he very often has to rely upon well-balanced judgment and experience in the industry, and an intimate knowledge of the job itself and its place in the organization he works for. He must, therefore, come into close contact with the other activities of his company: with production, with transport, with refining, with marketing, with finance; even, today, with what is perhaps more important—matters of public relations and employee relations. Indeed, I suspect that the industrial research man knows more about what is going on inside the company (a good man does) than the whole board of directors.

This makes life very interesting, but it does put a lot of responsibility on the individual. He has to be, for instance, his own policeman; and exercise self-discipline so that he concentrates his energies in the most economic manner. He must also be able to ignore sidelines which do not have a direct bearing on the project in hand, and, harder still, be able to break off his study, perhaps at a very interesting and promising stage, when it appears no longer practical in its application. I am sure Dr Williams knows as well as I do how easy it is to throw good money after bad, and above all, this industrial research man of ours must be able to work under the stress of the clock—"of the calendar" is perhaps a better way of putting it. It is no use spending a couple of years perfecting a new development if, in that time, he finds that his competitor has done the job, perhaps a little less thoroughly, but a little sooner and beaten him by three months to the market.

I think Dr Williams will agree with me that all industrial research problems can divide themselves into three stages. First, there is the stage of defining the problem; secondly, solving it; and thirdly, selling the solution to the people who are going to use it. I think they are all equally important, although the middle one—the actual job of solving the problem—is perhaps the least time-consuming, and the least heart-breaking of the three.

Applicational research fits into this general pattern. We know, for instance, taking one of Dr Williams' examples, that motor car valves will burn, but how do they burn and when do they burn? And in what circumstances do they burn? That is defining the problem. Time does not allow us to get all the information we need. A tremendous amount of work has been done on that particular run and we have still barely scratched the problem. This is a common state of affairs in industrial research as a whole, and perhaps most common in this field of applicational research. It is the reason, I suppose, for personalities playing such a large part in product development. We have to rely in the first place on personal interpretations of inadequate data. Personal opinions will differ on what is good and what is bad, and even personal prejudices, too, often play a part.

So we start off on the business of solving a problem while it is still only half-defined. We take some time considering experiments on equipment which, incidentally, becomes out of date while we are working with it (a very serious difficulty, which we know no way of overcoming), and we form some notions on what may be the important factors. What amount of lead should be tolerated? What about the conventional evacuants? Does the hydrocarbon composition of the petrol, and the sixteen other additives which it contains, affect the picture? What about the lubricating oil? The problem goes on changing while we are working on it, and we chase the changing problem with a changing remedy, and all the time the refiner is looking to the research man for guidance on how to design his product; the marketer is looking for guidance on what he should sell, and what he should say when he sells it. It is at this stage that personalities count most. There is no right or wrong solution to these problems at this stage. There are probably a dozen possibilities which we could have chosen from the data available. In such a case, the opinion of the man of experience and with the reputation is likely to be the one that is accepted. At the risk of spoiling my point, it is small wonder that different oil companies offer the motoring public different routes to happy motoring.

I have referred to the three stages in an industrial research problem. The stages of defining a problem, of solving it, and then of putting over the solution. It occurs to me that applicational research, in itself, is stage one of a broader problem in the industrial research laboratory, of the petroleum industry, at any rate. In its final analysis, applicational research is the specification writing for the products of the future. Doing it tells the refiner what is going to be needed of him. Of course, the research man cannot leave the job there. Somewhere in the research organization the next stage, that of solving the problem, must be tackled; this means finding out how products to meet the specification can be made, from the crude oils available, and that is how we get on to processes at this stage.

Now, perhaps, it is in this unique characteristic of applicational research being stage one in the broader field that we come across a difficulty; at least, a difficulty in my own experience—that of finance. Dr Williams touched upon this at the beginning of his paper and in one of his early slides. He told us, in a general way, of the factors which are considered in determining expenditure on research, and one of his factors was the amount of finance available for the job. He does not tell us how, in his organization, the expenditure is justified. How, for instance, does he convince his shareholders that such and such a total expenditure is the right one? How can he show a pay-off from the investment in any one field of applicational research? I know that is a difficult question to answer. In other fields of research, such as research on refinery processes, it is quite easy to show, over a period of years, whether the work is fruitful or not. Development in the process field leads to patentable inventions, which are tangible assets; it leads to the development of whole processes which can be bought and sold and, indeed, are bought and sold in research organizations and refineries at prices which can be worked out by both parties, by an assessment of the relative value of feed-stock and the products, balanced against the capital and operating costs involved. You can do much the same thing with many inventions in the transportation field and the production field, even in the exploration field, but very rarely, in my experience, can you do such a thing in the field of applicational research. We have, in fact, no such yardstick. We do not offer to produce patentable inventions; we rarely have something tangible that you could go along and sell to another party, and thereby establish a price or a value for your work.

Furthermore, the results of applicational research are of interest primarily to the marketing side of the organization, and marketers, again from my own experience, are notoriously bad prospects for money. They have never any money to spare. They are not in the habit of looking to the long term for return on expenditure: they are looking down the road for a return on any investment they might make. They will spend £50,000 on an advertising campaign for a new product, but to ask them to spend £50,000 on a research project which may give them a new product three years hence, is asking too much. Nevertheless, if the money

is to be spent, it has to be found, and I, for one, would be very interested in any comments which Dr Williams could make on this aspect of the job.

Dr C. G. Williams: I agree with all Mr Windebank said about the personal qualities required in research workers. He also asked a question about the justification of applicational research: how to prove to the management and shareholders that it has a pay-off. Of course, the answer is that research just cannot be justified in such terms. It is true, as Mr Windebank says, that on the process side, it is possible to say what a given research achievement is worth in terms of licensing fees, or licensing savings, but applicational research, is only one of the many activities, such as marketing and advertising, of an integrated oil company. If any one of these activities is discontinued, one soon notices the difference in one's business. I am not suggesting, of course, that anybody should stop research to find out how much his business suffers as a result. In actual fact, a good deal of research is carried out to retain existing business, and that is one of the main justifications for applicational research. I find that although the marketing side may be unwilling to foot the research bill, there is always plenty of pressure from them to go on with applicational research.

D. Downs: I am interested in the question of division of effort in a research laboratory between the more fundamental studies and the more *ad hoc* ones. Some eight years ago, two very informative lectures were given to the Royal Society in London by the directors of research of the Eastman Kodak* and Bell Telephone† organizations. Both these companies pursue very comprehensive and liberally-conceived research programmes, and their directors' estimates of the proportion of their research effort devoted to fundamental studies, varied from 25 to 30 per cent. Dr Kenneth Mees, of Eastman Kodak, in particular, made the point that their policy of giving absolute freedom of enquiry to a proportion of their research workers, paid off in terms of new discoveries, the commercial returns from which more than covered the original expenditure.

Apart from the intrinsic value of any discoveries which may be the direct result of fundamental research studies, the better appreciation which they give of the background of any subject, infuses the more *ad hoc* investigations, and enables them to be planned and carried out much more intelligently and economically. If I may take as an example a subject in which I happen to be particularly interested—engine combustion. We are often asked to study particular aspects of the burning of a fuel in an engine, knocking combustion, pre-ignition, and running-on in the spark ignition engine; cold starting and combustion noise in the case of the diesel engine; and combustion efficiency and weak and rich mixture ignition limits in the case of the gas turbine engine. It is very easy to study these problems, devise techniques for assessing them and ways of overcoming them, without realizing that although they are very different, they are nevertheless related, and are, in fact, all a part of the general problem of the oxidation of fuel in air. Although we know quite a lot about fuel oxidation, mainly from university work, the field is wide, and there is a great deal more to be done if we are to place all the engine combustion phenomena inside the wider framework of general knowledge on this subject. I wonder whether the oil companies spend as much of their research budgets on these and other fundamental phenomena related to their product, such as friction and wear, as they might. In other words, are they as liberal in their research policy as the two great American companies that I have mentioned? I should be very interested to have Dr Williams' views on the proportion of the oil companies' research expenditure devoted to fundamental work, and whether he feels that this could usefully be increased.

Dr C. G. Williams: I think the limiting factor is frequently not the amount of money which a laboratory is prepared to spend on fundamental research, but on the number of good ideas. It is all very well to say that it is a good thing to work on combustion, or on some other general subject, but there has to be a more specific idea of what is worth studying and in so far as my own company is concerned, we have not reached the upper limit of what we are prepared to spend on good fundamental research. We spend money on fundamental research at the universities and in our own laboratories, but the amount is under 10 per cent of our total budget. We would like this figure to be higher, but this is a sphere of research where it is necessary to have the right kind of people with the right kind of fundamental approach and with good ideas.

D. L. Samuel: I have been very interested indeed to listen to such an authoritative talk this evening because more nonsense is generally talked about research than about any other subject.

There is much glamour surrounding research—particularly so far as the layman is concerned—although it is evident among some scientists also. Many chemists fresh from taking their first degree at a university try to sell themselves to industry as research workers, and the same may be true of physicists and engineers.

I have always thought that research was one of the easiest things to start but the most difficult to stop. Mr Windebank said that one of the qualifications necessary in an industrial research worker was the ability to know when to stop. However, in my experience, the research worker will never stop of his own accord. He gets interested in his investigation for its own sake, and so may lose sight of the reason for doing the work. He will wish to pursue it perhaps long after the need for it has passed. Most research workers have to be forced to stop, and it must be one of the hardest jobs of a Research Director to order his team to suspend operations on a much-loved investigation.

I should like to ask Dr Williams whether any financial limitation is imposed on a project when it is initiated. I presume that when a problem is submitted for research, someone must consider first whether the work involved is worth carrying out. Having accepted the problem as a research matter, does anyone then decide how much money is worth spending to achieve the desired results? I appreciate that such a matter might have to be reviewed from time to time according to the progress of the investigation, but it would seem to me to be important to have an idea before beginning of how much money should be spent.

In carrying out applicational research, the initial work is normally done in a laboratory. When the laboratory has gone as far as possible the investigation may have to be carried further, outside the laboratories. Presumably this outside work also qualifies as part of the research project—even though it may be in the nature of field trials.

The modern trend of research appears to favour the team rather than the individual. This would seem to me a far more economical way of handling investigations. In a team only the leader must be a born research man—a man full of ideas and imagination and capable of drawing up the plan of campaign. The rest of the team need only be competent engineers, physicists, and chemists capable of carrying out the leader's plans.

In conclusion Dr Williams spoke of the limitation of ideas on research. I am rather surprised at this remark.

* Mees, C. E. K. *Proc. roy. Soc.*, 1948, **A192**, 465.

† Kelly, M. J. *Proc. roy. Soc.*, 1950, **A203**, 287.

I have always found that marketing people were full of ideas as to how their products could be improved, and were teeming with suggestions of research that could make their lives easier. Many of these ideas may not make practical research projects, but surely from this prolific source a number of worth-while research problems must be forthcoming.

Dr C. G. Williams: I agree with what Mr Samuel said about the difficulty of stopping unproductive research. Those who have had to perform this unpleasant task have the feeling of being guilty almost of infanticide.

Mr Samuel asked whether it is possible to predict in advance how much should be spent on a particular project. Such an estimate may be attempted, but, more usually, one decides on the intensity of effort per annum. Reviews are made at intervals on the progress made and the amount spent, and it is then decided whether it is worth continuing.

Mr Samuel commented on my reply to Mr Downs on the limitation of ideas on research. I agree that there is no limit on the number of suggestions which may come from all sorts of people, but a distinction should be drawn between the number of suggestions and the number of good ideas. I do not wish to suggest that there is always a shortage of good ideas, but this is a factor which sometimes limits the effort in one particular field, particularly in that of fundamental research.

J. G. Withers: A research organization should not be too rigid; and in many of its features flexibility is very desirable. Three important examples of the value of flexibility are given in the paper. The first is flexibility to allow the research worker, on suitable occasions, to go out to observe for himself the conditions of product utilization and the results of any untoward behaviour of the equipment or the product. If he receives this information secondhand it is very likely that important points are missed.

The second point is flexibility to allow physicists, chemists, and engineers to work together on a project rather than be segregated. A much greater width of experience can then be brought to bear on the problems in hand.

The third aspect of flexibility is to permit a small number of machine tools and drawing boards outside the highly efficient centralized workshops and drawing offices for the benefit of those research workers who wish to use them.

Finally, there should be flexibility to let the individual work out his ideas for himself without too rigid a control.

On the question raised by Mr Downs relating to the percentage of fundamental work in a research establishment, I think Dr Williams' figure of 10 per cent is a fair indication of what is actually taking place at the moment, but I do not agree that the limitation is set by the number of ideas that are forthcoming. I think that the real difficulty in increasing this figure is in drawing a hard and fast line between the equipment and men to work on fundamental work and on day-to-day problems. The day-to-day work is always very pressing and at the time good reasons can usually be offered why fundamental work should temporarily be stopped to speed up another programme. The solution is in being firm in the decision to allocate a fixed percentage of men to fundamental work regardless of the pressure of other work, and this percentage should, in my opinion, be at least twenty.

B. S. Brailey: Among the many other requirements of successful research, Dr Williams referred in passing to the question of availability of suitable man-power. This question is, of course, of the greatest importance, and Dr Williams may well have wished to deal with it at greater length than the time for his paper would allow.

Applicational research frequently has to solve problems arising from the operation of industrial plant. The ideal arrangement in such a case would be for research to be handled, wholly or partially, by the man who has encountered the problem and who has close familiarity with it.

Unfortunately, in the normal course of things, this man would not be available for research work, nor would he necessarily have experience in specialized research technique. The result is, therefore, that continuity of work on a problem is lost.

A similar position arises in the reverse situation, after a possible solution to the problem has been evolved in small-scale research, and this solution is handed over to the industry.

By that time it is the research man who is familiar with the background of the new method to be adopted, but the work of translating that method to full-scale operation will be done by other people, thus again breaking continuity.

The requirement of continuity of research and its industrial application is fundamental, and it would be of great interest to learn how matters stand in this regard.

Dr C. G. Williams: I agree that one of the main problems is the actual application of research, and that ideally, the research man, with his enthusiasm for his project should be the person to see it through. But only too often, because of lack of training and experience and the difficulties of moving a man from one country or laboratory to another, this cannot be done. What we are doing in a small way, and to an increasing extent, is to give research people wider operational experience by transferring them for short periods to other departments, *e.g.* marketing or manufacturing. I think more of this should be done, and that it will help in the application of research.

R. A. Fraser: A subject of special interest to the petroleum industry is applicational research as applied particularly to marketing, in other words, the application of the products. Many of the problems which come back to a research department in that connexion arise from questions which are put to the sales people in the field, and they are not necessarily the people best qualified by their training or experience to deal with the thing in a technical way, so it comes back to the technical department. It may be something which can be answered from the experience of the technical department, or it may require possibly quite an extensive research programme to answer the question. The ideal way would be for the research worker to go out into the field, as Dr Williams suggests, and investigate the problem there, but there are a number of cases where the size of the problem would not appear to justify that, especially as it means taking the research worker out of his laboratory, and possibly taking him off a job in which he is already deeply involved. It seems to me, therefore, that there is scope in a research organization for what might be called a technical liaison officer, who can act as a liaison between the sales force and the research worker. I would like to know what Dr Williams thinks of that idea.

Dr C. G. Williams: I agree with Mr Fraser's suggestion, since it is carried out by my company, and probably by other oil companies also. If there is a technical advisory department, which is in close contact with marketing, it is in a position to sift the field evidence, and to bring in the research worker at the appropriate time when the evidence is sufficiently clear, thereby avoiding wasting the time of the laboratory staff.

C. W. G. Martin: A number of speakers appear to be worried by the problem of knowing just when to break off applicational research, and what to do about it when

you have. Is not that where one ought to turn to the people whose job is fundamental research? I appreciate the problem of carrying a load of more than 10 per cent (or whatever it may be) of so-called fundamental research workers in an applicational research laboratory, but if a project gets to a point where the research worker, or perhaps it would be more true to say those behind him directing the work (and paying for it!), can see no early prospect of a solution to the problem, is not that the point where the investigation might be taken out of the field of commercial or applicational research and dealt with from a fundamental or academic point of view? In fact, does this not then provide a field for work at the universities, who might, at relative leisure, keep the problem ticking over while they can acquire more fundamental knowledge, the lack of which, I suggest, may have been the reason why success was not achieved in the first place. With such additional fundamental work completed and the world richer for such knowledge, there might then be a prospect of success by the applicational research workers if the same problem or a similar one were still worrying the industry.

Dr C. G. Williams: I have been trying, without success, to think of an investigation which we have stopped, or tried to stop, and which it would be appropriate to hand over to Professor Morton. It is possible that some investigations might be suitable for fundamental study at a university, but more often they are probably not the kind that one would like to hand over to some other organization, since, in an applicational laboratory, the problems are normally very close to the marketing side.

Prof. F. Morton: From the university point of view, the first point that struck me in Dr Williams' statement in presenting his paper was the £3000 per employee working cost for investigation in industry, with £5000 laboratory costs to support him. It makes me feel that the actual cost of research in the universities, which involves roughly £400 per man, and perhaps £150 grant to maintain the laboratory in which he works, is not such an expensive way of doing research after all.

I would like to raise very seriously this point of fundamental *v.* applicational research. Mr Windebank referred to people in applicational laboratories as being looked upon as the poor cousins of people doing so-called fundamental work, and I think if that is the case, it is their own fault. Applicational research is not sold to research-trained personnel in the universities in the correct manner. It seems to me that too big a distinction is made between fundamental and so-called applicational research. The only difference, as I see it, is that both involve the application of scientific method to solve a problem. It does not matter what the problem is, provided that it is sufficiently interesting for somebody to pay to have it solved. As far as the research worker is concerned, if he can be interested in the problem, there is no distinction, provided it is the truth that is sought. The academic worker has a time limit. It is perhaps unfortunate that university research in the past two centuries has been controlled to some extent by the Ph.D. system, which involves a time factor to a research worker. It has delayed the ideal development of team work in a university, and it is because of the growth of applicational research in the universities, as a result of war-time efforts, that we have been able to develop team research in universities.

Another point raised, was that in industrial research the man must discipline himself. Now, the fundamental idea of university research is self-discipline. The research worker must plan his work and utilize his time in order to complete the particular problem on which he is working within a reasonable period. He is helped in this by his supervisor, but if he does not do the work, he does not get the degree for which he stayed. The too rigid application of discipline, particularly in regard to hours of work, etc., can, in fact, be harmful. It is obviously necessary to maintain records of attendance etc., but allowance should be made to permit the external reading so necessary to maintain a research worker's interest in his subject, and to prevent his taking too narrow a view of his own problem.

Most of the applicational work which I have seen discarded by industry has been abandoned for good and proper reasons, and it is unlikely that any real fundamental advance in knowledge would have come from the completion of the work. Normally, it is unlikely that it would be of benefit to the industry to return such a problem to a university. But I feel that in many of the problems which are discarded, there may be the nucleus of an idea for fundamental development, and before such projects are discarded they should be very carefully reviewed to see whether there is the possibility of development from those results. If the project is not completed it is normal not to publish the results, and somebody else may start on the same problem without knowing that it has been attempted and shown to be unlikely to yield results of value.

Dr C. G. Williams: I would like to point out that the figure I gave was not £3000 per employee but £3000 per technician or scientist. The latter usually has one or two other people to help him and, therefore, the cost per employee is somewhere between £1000 and £1500. The £400 or £500 per research student at a university does not include many invisible aids, such as university grants, professors' salaries, capital depreciation charges, and so on. The figures are not, therefore, comparable.

In regard to stopping a research, a particular investigation may be quite sound. What frequently happens is that circumstances change for marketing or other reasons and the objective is no longer worthwhile. The enthusiasm of the research worker remains, however, and he may feel that it is a great shame that a business concern imposes commercial considerations on him.

Prof. F. Morton: It now remains for me to express on your behalf our thanks and gratitude to Dr Williams for coming here tonight, and I ask you to show your appreciation in the usual manner.

The vote of thanks was accorded with acclamation.

TRENDS IN EARLY INSULATING OIL SPECIFICATIONS*

By E. A. EVANS (Past-President)

INTRODUCTION

At the request of the Electrical Research Association trends are recalled in this paper of the early work of the Insulating Oil Committee, which was called together originally by the Institution of Electrical Engineers when mineral oils for oil-cooled transformers were in their infancy, and sludging troubles caused anxiety. In those days very little was known about oil oxidation, consequently much of the work done was of an exploratory character and not sufficiently finalized to be published except as reports to members of the Association. The present account is based upon E.R.A. papers, and jottings are included where necessary in order to make the subject interesting and readable.

SLUDGE TROUBLE

The first mention of oil being used for insulation appears to have been made in a U.S. Patent granted to Moody in 1906. In 1910, Digby and Mellis read a paper before the I.E.E. on the use of oil in electrical transformers, and the care which is necessary in selecting it. Duckham, in 1911, gave his views on sludge formation, using ozone as the oxidizing agent. Then, in 1913, Michie presented a paper to the I.E.E. He showed that the sludge in transformers closely resembles the sludge produced in oil which is air-blown. The next paper of historic interest was by Brauen, in 1914, who suggested that the action of oil on metal was really due to generated acids.

Sludge troubles were so important in those days that in 1916 the I.E.E. appointed an Insulating Oil Subcommittee. Later, the work was transferred to the British Electrical and Allied Industries Research Association. The first duty of the committee was to investigate the effects of temperatures below the 150° C which had been selected by Michie for his oxidation test. Accordingly, 60°, 70°, 80°, 90°, 100°, 120°, and 150° C were chosen for comparisons. The results varied so widely that the only conclusions reached were that values increased with temperature. In those days it was thought that generated acids in some way increased the sludge value of oils. Investigations revealed that added organic acids increased the sludge, but the cause was attributed to the action of the acids on the copper catalyst used in the test. A natural step was to examine the influence of the condenser in the test apparatus for retaining the generated volatile organic acids. Soon considerable controversy arose over condensers between the various national committees who had interested themselves in the subject. Arising out of this investigation, it was noted that formaldehyde is a product of the oxidation. That formaldehyde was an intermediate product in the combustion of hydrocarbon gases had already been observed by Dixon.

Clearly, air, oxygen, or ozone might be used for the oxidation, but would the results be the same? Ozone was quickly eliminated owing to its extreme reactivity. Oxygen produced sticky deposits which were difficult to handle. The conclusion was that the oxidation was different from that when air was used. It had not occurred to the experimenters that the stickiness was due to the greater production of acids, and the subsequent formation of metal soaps which can be extremely sticky. The stickiness was especially noticeable when different metal catalysts were used. It may be that Radcliffe, who was engaged on the work, suspected it, although he never stated it. Certainly, he stressed the importance of acidity determinations after sludging. Now his suggestion appears in the latest specification.

All kinds of tests were made to sort out possibilities, such as refractive index, coefficient of expansion, methoxy test, formalite numbers, iodine value, etc. During the oxidation tests the production of water, carbon dioxide, and acids had been observed, but what part they took in sludging was not known. They therefore became jumping off grounds. A programme was undertaken to estimate the amount of carbon dioxide and water which was produced, together with the loss of weight of the copper. Almost at the outset troubles were encountered. The original six tests soon ran into fifty-two before a reasonably satisfactory method was devised.

In defence of any programme which was agreed after the first world war, it must be remembered that the only methods for oil refining were sulphuric acid and earth treatment. Very little was known about the composition of the constituent hydrocarbons, and less about the mechanism of their oxidation. It was to be expected therefore, that much sieving was necessary to produce a test which did, in fact, resemble field conditions, and which was reproducible in different laboratories. Let it be remembered that the original Michie apparatus has had very little structural modification, and only a few operational improvements. It must also be remembered that sludging troubles were the "roots" of the oil investigations. The necessity for removing them compelled the oil industry to concentrate on more efficient sulphuric acid refining. This was done, and sludging troubles disappeared when Class A oil appeared in an atmo-

* MS received 7 December 1954.

sphere of opposition. However, much as one may wish to forget the opposition, some of the detailed investigation was doubtless destined to emphasize the good quality of Class A oils.

In 1892 Holde found that with hydrocarbon oils under the influence of oxygen their iodine values were reduced, showing that the unsaturated compounds had been converted into saturated compounds. In his experiments no increase in acidity was noticed. Aisinmann, in 1895, postulated that the action of mineral oils on metals must be due to organic acids, but it was not until 1904 that Zelinski prepared organic acids from petroleum, by chlorinating paraffin with the aid of a catalyst and reacting it with magnesium. By 1908, Charitschkow had prepared acids by direct oxidation. To him they were naphthenic acids. In the same year Molinari and Fernardi prepared ozonides by the use of ozone. These products, which were white or pale yellow, were soluble in ether, chloroform, and benzene. They decomposed rapidly at 20° C, yielded a soft red substance at 45° to 50° C, and a brown resin at 150° C.

Prelescdazew, using an organic peroxide, obtained what he thought to be oxides of unsaturated compounds. The work of Waters in 1911 laid special stress upon the deposit which was formed under the influence of heat and air, which he ascribed to oxidation. He further observed that water and carbon dioxide were given off, and that acids were formed. In 1912 Wilson and Heaven described a method for measuring the rate of oxidation which consisted of exposing oil on kieselguhr, to give the maximum surface area to oxygen, and noting the increase in weight. The observation of Istrate and Teodorescu, in 1912, that copper salts are produced when copper is kept in oxidized oil is of special interest.

These historical recordings make it clear that there was some background for the I.E.E. investigators to guide them.

From then onwards papers on oxidation increased. The many references to iodine value and unsaturation made it abundantly obvious that unsaturated hydrocarbons were regarded as the oxidizable material. Radcliffe stated that the opinion has long been held that there is a chemical relationship between the iodine value of an oil and its sludging propensities. Little wonder, therefore, that some people hung on so tenaciously to iodine value for so long, in spite of a devoted opposition.

Other countries besides England were, of course, interested in transformer oils. France was probably the first to issue a specification, even though it was a very simple one. With the transformer oil plant at Hamburg refining Russian oil, it was to be expected that Germany should also interest herself; and Sweden and Switzerland with large electrical undertakings pursued the subject. The U.S.A., with its dual interests, had its findings published by the ASTM. These several countries had their own special tests, each being guarded by national pride. Through the I.E.C., efforts were made to agree upon an international test, but as prejudices were so strong they failed, as so many other efforts have done.

In 1930 the E.R.A. issued " Report on Investigations on Tests Recommended by Various Countries for the Determination of Sludge in Insulating Oils." The American, British, German, Swedish, and Swiss tests were critically examined. Tests were made at the specified temperature, and at a common one of 110° C. The work was really undertaken to enable the I.E.C. to adopt and standardize a method for research purposes only. Various types of oil were used. The report can be summarized as follows:

American Test. A number of oils were heated at the same time in a rotating oven at 120° C, and regularly examined for the first trace of sludge. It was found that the " life " of any specific oil depended upon whether it was heated in the presence of other oils of shorter or longer life, or by itself.

German Test. Oxidation by oxygen at 120° C for 70 hr, and finally extracting the tar acids and weighing them. The results were not very consistent. Further, it only measured the non-volatile acids, as no condenser was used.

Swedish Test. Oxidation by oxygen at 100° C for 100 hr. A 10,000-V electric stress was maintained between copper electrodes. Great difficulty was experienced in operating the apparatus, and it is doubtful whether the electric stress played any useful part.

Swiss Test. Oil heated in copper beakers at 115° C in which cotton thread was immersed. After 168 and 336 hr the tensile strength of the cotton was determined. Repeatability was not good.

It was noted in the report that when using the British test low sludge was usually accompanied by high acidity, and high sludge with low acidity. Since then, of course, things have changed.

Snyder, who designed the life test, wrote in an ASTM publication, in 1924, that he was satisfied that oils oxidize, and that when oils start to sludge there is an increase of core temperatures. Hence his reason for observing when oil commences to sludge. He stated that the Michie test does not measure the ability of an oil to resist oxidation, but the ability to resist the catalytic effect of copper. He felt that this was important, as the ratio of exposed copper to the volume of oil was very small.

The year 1917 will seem to many to be a long time ago, indeed too far away to marvel at any discovery which was made in that year. Yet it was an eventful year, for in it Hirobe discovered that the break-down-value of an oil was mainly influenced by water and fibres. It is all now so commonplace that the name of Hirobe is almost forgotten. Hirobe worked at the Electrotechnical Laboratory of Tokyo. He announced that oil could be purified to give a B.D.V. of 90,000 by careful centrifuging, and that added moisture alone reduced it to 60,000; but when fibres were incorporated with the wet oil it fell rapidly. This was the foundation upon which future electrical tests and discussions were based.

Strangely enough, the need for water estimation had not been seriously felt, at least not in small quantities. Loss on heating in an oven was used, also drying on a roll of absorbent paper such as was used in milk analysis. The Admiralty had found the need to estimate the water content in fuel and turbine oils during the first world war, so Philip designed his well-known method for moisture estimation. Unfortunately, it did not prove to be sufficiently sensitive for transformer oils. What degree of sensitivity was required was not really known, so it was not surprising that in 1920 the E.R.A. suggested that work should be commenced on the relationship between moisture content and B.D.V.

With the limited knowledge available, some were bold enough to state that a B.D.V. of 25,000 was too low, and that preliminary sparking should condemn an oil. This indication of the strong views held in those days makes one marvel that sufficient agreement had been reached in 1921 when the first proof of British Standard 148 was printed. They were stormy days, both technically and commercially, and the position was aggravated by similar strong-minded gentlemen in Germany, Switzerland, and the U.S.A. In the U.K., Class A oils made from Russian crude were unflinchingly upheld, while other countries had quite different views, which were freely expressed. As this part of the present paper is devoted to B.D.V., it is only necessary to refer to the fact that sphere electrodes had been used in the U.K. and disk electrodes in the U.S.A. In 1922 there was some correspondence between the E.R.A. and the ASTM which resulted in the E.RA. using spheres, and the ASTM using disks. Everest and Raynor, who were principal British advisers on this subject, decided that a thorough investigation should be made. When this was done it appeared that spheres were correct. The following year (1923), added support came from Hirobe, who published a paper entitled "Effect of Dielectric Field Produced by Disc and Sphere Electrodes on Distribution of Impurities in a Dielectric." Tables I and II are taken from that paper.

From the data in the tables it will be observed that with oil containing added moisture only the values obtained with sphere electrodes were 18 to 25 per cent higher than values obtained with disks. When fibres were introduced the two sets of electrodes gave identical results when the oil was free from moisture; but as soon as water was added the disk electrodes gave values 3·15 per cent higher than those obtained with spheres.

The greater sensitivity of the spheres to moisture plus fibre is probably due to the lining up of the wet fibre on a line which is the shortest distance between the spheres. Whereas when disks are used the particles are drawn into the strongest portion of the electric field which is generated from the edge of the disk. The subject was too delicate to be left as it was, so Fleming was consulted. From the practical work which was done by Jackson, he upheld the British view.

Fortified with the added knowledge, the E.R.A. submitted to the B.E.S.A. (now British Standards Institution) in 1923 a sketch of the B.D.V. apparatus which had been carefully designed to incorporate ½-inch spheres. The apparatus had been found to be safe up to 60,000 V by Fleming at University College, London. The transformer set was of necessity expensive, and only suitable for test house operation.

TABLE I

Comparison of the Effect of Moisture Content of Fibre-free Oil on the B.D.V. Obtained with ½-inch Spheres and ½-inch dia Disks, Each Arranged Vertically

(Gap = 3·81 mm)

Water added (p.p.m.)	B.D.V. at room temperature		Percentage decrease in B.D.V. from that of moisture-free oil	
	Spheres, kV	Disks, kV	Spheres	Disks
0	90	67·5	0	0
40	71	57·5	21·0	14·8
80	66	53·0	26·6	21·5
120	64	51·5	29·0	23·6
200	62	50·5	31·0	25·1
280	61	50·0	32·4	25·8

TABLE II

Comparison of the Effect of Moisture Content of Oil Containing Fibres on B.D.V.

(Gap = 3·81 mm)

Water added (p.p.m.)	B.D.V. at room temperature		Percentage decrease in B.D.V. from that of moisture-free oil	
	Spheres, kV	Disks, kV	Spheres	Disks
0	35·0	35·0	0	0
40	20·0	22·5	43·0	35·6
80	16·5	19·0	53·0	45·5
120	16·0	17·5	54·0	50·0
200	15·5	16·5	55·8	52·8
280	15·5	16·0	55·8	54·0

Consequently, in a very short space of time Empson appeared with his dielectrimeter operated by an induction coil. Immediately the E.R.A. suggested that as a transformer set gave a sine wave, which the dielectrimeter did not, it was not to be expected that the two forms of test would give identical results. Furthermore, as the wave shape of an induction coil is changed by any alteration in the interrupter, and probably also by the setting of the air gap used to control the voltage, there cannot be any constant relationship between the B.D.V. obtained with an induction coil and a transformer. The undaunted Empson, knowing the limitation of the transformer set, proceeded with the dielectrimeter, and found many willing buyers who required either a medium-priced article or one which was portable.

Empson will also be remembered as a pioneer of the oil centrifuge. In fact, it is believed that he introduced the water washing system. Due credit must be given to the De Laval Company, who suggested, in 1922, the use of the centrifuge for purifying oil, as it gave as good results as the blotter press. Although these gestures are given ungrudgingly, it must, however, be acknowledged that the centrifuge had been in use in research circles previously.

The year 1925 seemed at the time to be a particularly happy one. At the I.E.C. meeting at The Hague it was decided to ask all national committees to draw up a list of tests, and figures to include in an international specification, on which it was considered essential to obtain international agreement. The great expectations never materialized. In 1954, about 30 years later and about the same number of years after another major war, the same drill is being enacted, but with more mature judgment.

The growing demand for sphere electrodes induced the ball manufacturers to advise the E.R.A. in 1926 that they did not make balls with a diameter of 12·5 mm, so the specification was changed to 13 mm.

Up to 1928, contentment had been found in the B.D.V. and its implications, but in that year doubts appeared about its universality, because wet oil could pass the specified value. Fibre-free oil could absorb water from a humid atmosphere and render it a potential danger in a dusty atmosphere. For this reason it was suggested that a crackle test should be explored. Cogle reported upon his determinations:

Water in oil (p.p.m.)	B.D.V., kV
0	97
2·3	97
11·4	94
22·5	84
53·3	75
103	75·7
187	79·5

He observed a crackle, even with oil having a B.D.V. of 94. Corroboration was given by the German Committee of the I.E.C., and in the *Journal of the Ateliers Constructions Electriques de Charleroi*. Discussions on the crackle test continued until 1930, when it was decided not to pursue it further, but owing to pressure from the B.S.I. it became necessary to reconsider the decision. The E.R.A. maintained its opinion, but mellowed it in 1934 by suggesting that, although it opposed its inclusion in the specification, the crackle test could be used on site as a safeguard test.

Anxiety about moisture caused many users to demand the ultimate B.D.V. in 1928. This appeared to be a modest request until it was remembered that the B.S. Specification stated that the value must be the highest voltage that the oil will withstand for 1 minute. This causes a difficulty, because each successive applied voltage should be maintained for 1 minute. Of course, it consumes time. A way out would be to step up quickly to 30,000 and to continue the application of the voltage at an agreed rate until an arc was established.

Up to 1930 attention had been confined almost exclusively to acceptance tests, but in 1930 a report was issued on a more applied aspect "Effect of Sludge and Acids on B.D.V." Oils were oxidized for various periods and the B.D.V. determined. Whether Class A or B oils of Russian or American origin are used, the B.D.V. falls to a minimum when the sludge content is about 0·2 mg/g, and then remains constant at about 47,000 for Class B oils and 37,000 for Class A oils. The new oil had a B.D.V. of 90,000. Added moisture, even up to 500 p.p.m. of oil in well oxidized oil only lowered the B.D.V. to 50,000. Even when the sludge is removed from the oil, leaving the acids behind, the B.D.V. in the presence of moisture falls to a minimum of 67,000. In the course of this investigation the acid and sludge production was plotted against time. The acidity of Class A Russian reached a maximum of 1 and Class A American over 2·5, at which both began to diminish, in spite of the presence of a condenser. Whereas Class B continued to rise throughout the long test, but the Russian at a much slower rate.

Class A American showed a rapid rise in sludge after 500 hr, but Class A Russian showed no such alteration, even after 600 hr. Class B American had a more rapid sludge production than Class B Russian without the sudden increase.

The target of the E.R.A. Committee was to recommend a specification to the British Engineering Standards Association to be published as a British Standard Specification. Accordingly, on 2 February 1921 a draft letter from E.R.A. to B.E.S.A. was circulated for approval, which it received. By April 1921 the first Draft of the British Standard Specification for Insulating Oils for Use in Transformers, Oil Switches, and Circuit Breakers was printed and circulated for comment, and in April 1923 it was first published under the Serial Number 148. A revision was published in 1927, and another in 1933, by which time B.E.S.A. had become the British Standards Institution. The present edition, which was issued in 1951, is under review, mainly in respect of the electric strength (B.D.V.) test, by B.S.I. Committee ELE/20, which is the British National Committee of the International Electrotechnical Commission.

Although the specification has not changed fundamentally since 1921, very considerable changes in the production of transformer oils have taken place.

A LABORATORY METHOD OF EVALUATING STREAMING POTENTIALS ACROSS MUD SHEATHS*

By N. C. SEN GUPTA† (Member) and C. G. BANERJEE‡

SUMMARY

The self potential (S.P.) obtained in Schlumberger logs comprises two potentials, one of which, the streaming potential, depends on the mud and the differential bottom hole pressure but is independent of the formation water. This potential has had to be deducted from the S.P. in order to calculate the formation water resistivity from S.P. logs. Direct measurement of streaming potential under high pressure is difficult. In this report a simple method of obtaining the streaming potential from endosmotic experiments is described.

THE self potential measured in the electrical logging of oil wells is made up of two kinds, which may be called the electrochemical potential (E_c) and the streaming potential (E_k). The electrochemical potential depends on the ionic activities of the connate water and the mud filtrate, and can be expressed approximately by a relation of the type :

$$E_c = K \log_{10} \frac{R_{xo}(R_s - R_o)}{R_o(R_s - R_{xo})} \quad . \quad . \quad (1)$$

in which K = a constant which depends on the valencies and, to some extent, on the mobilities of the ions, and bottom hole temperature.

R_{xo} = resistivity of formation saturated by mud filtrate as obtained from micrologs.

R_o = resistivity of formation saturated with formation water.

R_s = average surface resistivity of the formation when the solution resistivity varies between R_w and R_{mf}.

The surface resistivity (inverse of surface conductivity) enters into equation (1) because the specific conductivity of a solution in a capillary is different from that in the bulk phase.[1] The value of R_s depends on the nature of the formation and resistivity of the saturating solution; the value in many cases is of the order of 100 Ω metre. Values of R_s can be obtained from laboratory measurements on formation factors of cores using salt solutions of various resistivities. Results of such measurements will be submitted in a later paper.

The streaming potential depends on the resistivity and nature of the mud and on the differential bottom hole pressure. It is independent of the formation water resistivity. It is, therefore, essential to measure the streaming potential separately and to deduct the value from the total self potential (S.P.) so that R_o can be estimated, using equation (1).

According to Helmholtz, the streaming potential across a diaphragm or membrane, *e.g.* mud sheath, is connected with the potential of the electrical double layer round the colloid particles forming the diaphragm by the following relation :

$$E_k = \frac{P\zeta D}{4\pi\eta\kappa} \quad . \quad . \quad . \quad . \quad (2)$$

where P is the pressure difference between the two ends of the diaphragm, ζ is the double layer potential, and D, η, and κ are the dielectric constant, viscosity, and specific conductivity of the intermicellary solution. It is doubtful if the values of D and η obtained in the double layer are the same as those of the solution in bulk. On the other hand, it is reasonable to assume that the water dipoles are partially oriented in the double layer so that the dielectric constant of water in the double layer will be much less than the normal value for water. Further, surface conductance not accounted for in equation (2) appears to play a part, so that the conductivity term in equation (2) should be greater than the bulk conductivity of the solution. Some approximate calculations of E_k can be made from equation (2), assuming D and η to have the same values as those of water in bulk. At 20° C, $D = 80$ and $\eta = 0{\cdot}01$ poise. The value of ζ for clays appears to be of the order of 0·01 volt. Hence for a pressure difference of 100 p.s.i. and when specific conductance of the solvent is about 0·01 Ω/cm, the value of E_k appears to be about −5 mV. When the differential bottom hole pressure is of the order of 500 p.s.i., other factors remaining the same, the value of E_k can be as high as −25 mV. Although this method of calculating E_k is open to objection, it shows clearly that under certain conditions E_k can be 50 per cent or more of the total S.P.

Experimental determination of E_k under high differential pressures is difficult.[2] On the other hand, the value of E_k/P can be obtained very simply from electroendosmotic experiments using a relation originally deduced by Smoluchowski (1903). According

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to him, the following relation is obtained for a capillary :

$$\frac{E_k}{P} = \frac{V}{i} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where V is the volume of solution passing through the capillary in unit time when a current i is passed through it. The same relation also holds good for clay diaphragms or membranes as was shown by Saxen.[3] Transferring to practical units when E_k is expressed in millivolts, i in milliamperes, V in cc/sec, and P in p.s.i., the relation becomes :

$$\frac{E_k}{P} = 6{\cdot}89 \times 10^3 \frac{V}{i} \quad . \quad . \quad . \quad (4)$$

Equations (3) and (4) should be applicable under all conditions. In the absence of any surface conductance equation (3) can be combined with equation (2), giving :

$$\frac{E_k}{P} = \frac{V}{i} = \frac{\zeta D}{4\pi\eta\kappa} = \frac{\zeta D\rho}{4\pi\eta} \quad . \quad . \quad . \quad (5)$$

ρ being the specific resistivity of the solution. In the case of surface conductance when electroendosmosis through a single capillary tube of radius r is considered, equation (5) can be modified as follows :

$$\frac{E_k}{P} = \frac{V}{i} = \frac{\zeta D}{4\pi\eta\left(\kappa + \frac{2\kappa_s}{r}\right)} \quad . \quad . \quad . \quad (6)$$

In equation (6) κ_s is the surface conductance. Equation (6) has also been applied to diaphragms with some success.

A detailed discussion of the equations is unnecessary in this paper, but it should be noted that with a given diaphragm ζ, D, and the product $\eta\kappa$, are constants for any particular electrolytic solution, being almost independent of temperature. Therefore, E_k/P, for a given diaphragm, should be constant for any particular solution and should be almost independent of temperature. In other words, if the mud sheath is formed in the laboratory at a pressure comparable to the differential bottom hole pressure of the well and E_k/P is obtained across the sheath from endosmotic measurements, then the result multiplied by the differential bottom hole pressure gives the streaming potential under bottom hole condition. It must, however, be mentioned that sheaths formed at lower pressures would give a higher value for E_k/P, and hence a higher value for streaming potential when E_k/P is multiplied by bottom hole pressure. It has been shown elsewhere [4] that permeabilities of sheaths vary with the pressure at which the sheath is formed. With one mud the sheath formed at 100 p.s.i.g. showed a permeability of $5{\cdot}3 \times 10^{-3}$ mD, whereas the permeability of sheath formed from the same mud at 1000 p.s.i.g. was $1{\cdot}25 \times 10^{-3}$ mD. On the other hand, once the sheath has been formed at a high pressure its water permeability at lower pressures is found to vary only slightly with pressure.

Mud sheaths suitable for endosmotic experiments have been made using a high pressure tester [4] constructed in the Assam Oil Company workshop. The Baroid high pressure tester can also be used for the same. The sheaths are taken out and loose portions are scraped off before mounting the sheath on to the apparatus.

The electroendosmotic cell used is shown in Fig 1. The apparatus consists of two parts which are fixed

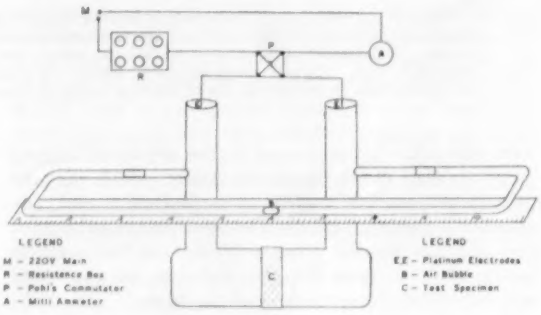

FIG 1

ELECTROENDOSMOTIC APPARATUS

on to the two sides of the membrane. The two limbs are connected by means of a capillary tube. After the membrane has been fixed in place the apparatus is assembled as shown in the figure, and filled with the test solution. An air bubble is introduced into the capillary side path. A direct current is passed through the solution by means of two platinum electrodes. The strength of the current is adjusted by means of a hand-regulated rheostat, and read by a multi-range milliammeter. The solution due to electroendosmosis tends to rise in one limb, and flows back into the other through the side path. This moves the bubble, and its rate of movement is measured with a stop-watch and a graduated scale. Knowing the area of cross-section of the capillary, the volume rate of flow can be measured, owing to the passage of any given current.

When a mud sheath is used as a membrane it has to be mounted in the apparatus with filter papers on either side of it. The effect of filter papers, if any, has been studied using a sandstone membrane. The effect of various chemicals has also been studied using the sandstone membrane. The air permeability of the sandstone membrane was about 1 mD and water permeability less than 0·01 mD being of the same order as that of average mud sheaths. The grain size distribution of the sandstone was :

	Per cent
Coarse sand (2–0·2 mm)	14·2
Fine sand (0·2–0·02 mm)	73·3
Silt (0·02–0·002 mm)	5·4
Clay (<0·002 mm)	6·0
Carbonates, etc. (by difference)	1·1

The clay present in the sandstone was of the montmorillonite–illite type.

Results of experiments made with the sandstone membrane with and without filter papers are shown in Fig 2. Sodium chloride solution at several dilutions was used as the migrating fluid in both cases.

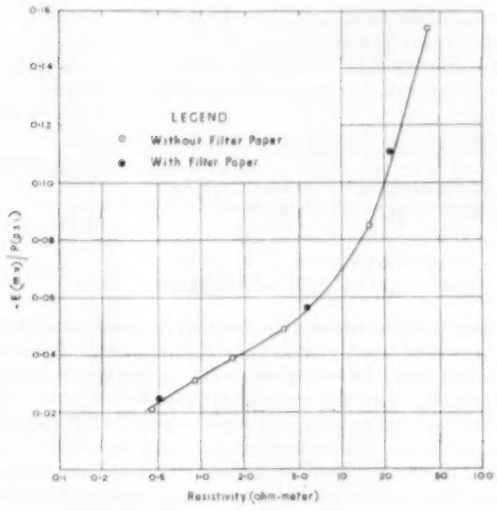

FIG 2

EFFECT OF FILTER PAPER ON STREAMING POTENTIAL

Resistivities are plotted along the X-axis and E_k/P in millivolts per p.s.i. along the Y-axis. The results show clearly that the filter paper has no measurable effect on the observed streaming potential.

The adsorption of cations depends primarily on valency. The actual size of the hydrated ion and its mobility in an electric field also influence the adsorption to some extent. In order to establish the effect of valency, experiments were carried out with solutions of sodium chloride, barium chloride, and lanthanum chloride using the same sandstone membrane. Results are shown in Fig 3; the inverse of concentrations of the electrolytes has been plotted against ratio E_k/P. It is observed that:

(*a*) Using sodium chloride the value of E_k/P gradually diminished with increasing concentration of the electrolyte. At sufficiently high concentrations the ratio tends to be very small.

(*b*) With barium chloride the values of E_k/P are lower than those obtained with sodium chloride at corresponding concentrations. At greater concentrations than shown in the figure E_k/P tends to reverse its sign.

(*c*) With lanthanum chloride the ratio E_k/P changes from a negative to a positive value at or about the concentration of $N/150$. At a concentration of $N/50$ the potential obtained with lanthanum chloride is almost equal and opposite to that obtained with barium chloride at the same concentration. With higher concentrations of lanthanum chloride the ratio again tends to diminish to a very small value.

In actual practice a trivalent cation is never added to well muds. A divalent cation, calcium, may be present as in red-lime mud or while drilling through anhydrite. In any case, results shown in Fig 3 clearly demonstrate that it is not possible to have a general relationship between the streaming potential and concentration or resistivity of the saturating solution which will satisfy all conditions.

All polyvalent and complex anions are known to be adsorbed by clay membranes, thereby increasing the negative charge on them. Among monovalent anions, only the hydroxyl ion is strongly adsorbed. Results of electroendosmotic experiments using sodium chloride, sodium hydroxide, and sodium hexametaphosphate (calgon) are shown in Fig 4. Since the calgon used was not very pure, resistivities rather than equivalent concentrations have been plotted along the

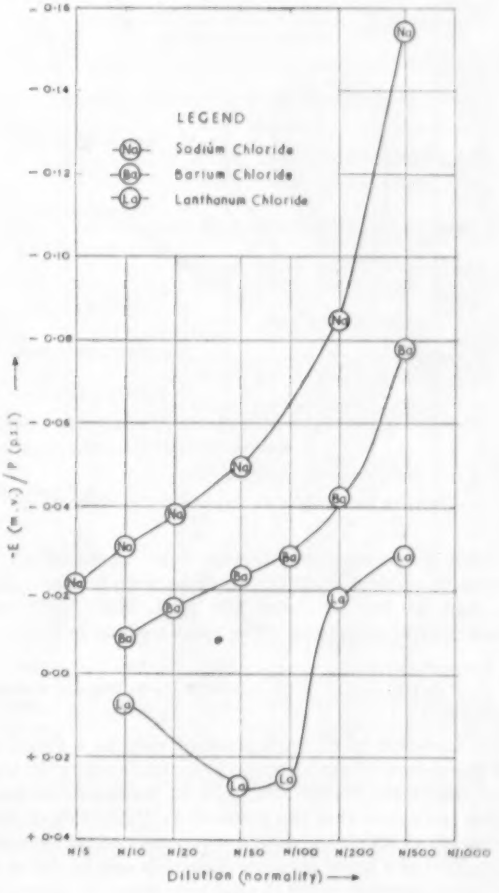

FIG 3

EFFECT OF CATIONS ON STREAMING POTENTIAL

X-axis of the figure. In the range of concentrations studied the results vary in the order :

Calgon > sodium hydroxide > sodium chloride.

Some experiments were made with sheaths prepared from a bentonite suspension and an ordinary drilling mud. Results were similar to those obtained with the sandstone membrane using sodium chloride as the saturating solution.

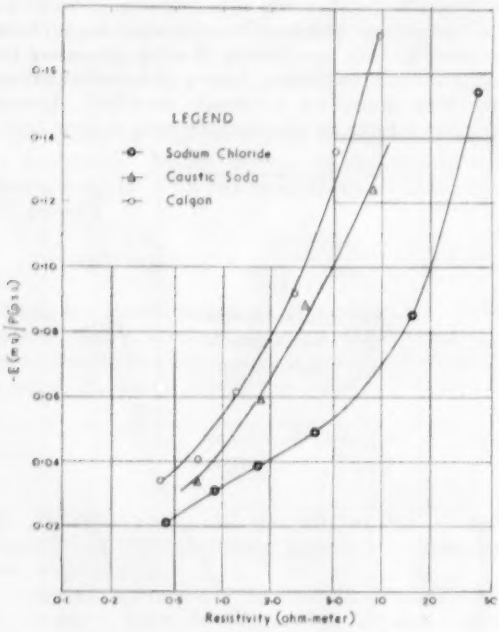

FIG 4

EFFECT OF ANIONS ON STREAMING POTENTIAL

Only a few experiments were done on the effect of pressure on mud sheaths. Sheaths were formed from a mud at 50, 150, and 500 p.s.i. and E_k/P was measured in each case. The results are as follows :

Pressure (p.s.i.) . .	50	150	500
E_k/P (mV/p.s.i.) . .	−0·1025	−0·0870	−0·0658
E_k (mV) . . .	−5·1	−13·1	−32·9

The variation of E_k with pressure may be a function of the nature of mud, but no systematic study on this has yet been undertaken. It is, however, evident from the above that the value of E_k/P depends on the pressure at which the sheath is formed and that results obtained at a lower pressure cannot be extrapolated at a higher pressure until the relation between E_k and P is known for the mud.

The streaming potential measurements were applied in the interpretation of a deep well S.P. log recorded in Eastern India. For this, the value of K in equation (1) was assumed to be 70 at the bottom hole temperature (180° F). The value of R_s was found to be about 100 Ω metre and the R_{xo} from micrologs about 21 Ω metre. The particulars are given below :

Bottom hole temperature = 180° F at 10,050 ft
Differential bottom hole pressure = 1100 p.s.i.
Deflection of S.P. surve from shale line = −68 mV
Resistivity of mud at bottom hole temperature = 3·0 Ω metre
Resistivity of filtrate at bottom hole temperature = 2·3 Ω metre
E_k/P (measured) = −0·04 mV/p.s.i.
E_k at 1100 p.s.i. = −44 mV
E_c (= S.P. − E) = −24 mV
R_{xo} (from micrologs) = 21 Ω metre
R_o (from equation 1) = 10·8 Ω metre

A check on this result was obtained from the resistivity of a water-bearing part of the sand. The value of R_o thus obtained was about 9·0 Ω metre which agrees reasonably with the calculated data.

On the other hand, if this observed S.P. is not corrected for the streaming potential then the value of R_o calculated from equation (1) is found to be less than 3 Ω metre which is less than one-third of the measured value. An accurate value of R_o together with the actual resistivity of the sand is required for calculating the percentage water saturation of the oilsand. Since the square root of R_o enters into this calculation, the use of 10·8 Ω metre instead of 9 Ω metre would introduce an error of less than 10 per cent, whereas the use of 3 Ω metre instead of 9 Ω metre will produce an error of more than 50 per cent.

It is thus evident that in deep wells especially when the resistivities of both mud and connate water are high the streaming potential constitutes the greater part of the S.P., and its neglect, or a large error in its estimated value, would greatly affect the estimated value for formation water resistivity and hence the deductions from the S.P. logs.

ACKNOWLEDGMENT

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ISOMERIZATION OF MINERAL OILS WITH HYDROGENATION CATALYSTS *

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(in collaboration with T. VAN DER HAUW and G. P. M. VERBEEK)

SUMMARY

The possibility of *cis–trans* isomerism of naphthenic mineral oils is discussed. Prolonged heating of several saturated lubricating oils of pronounced cyclic character with nickel catalysts and high-pressure hydrogen (50 to 200 atm, 250° to 300° C) causes remarkable changes of their physical properties, which may in all probability be attributed to *cis–trans* isomerism of the naphthenes. The isomerized oils show a small improvement of viscosity index.

INTRODUCTION

It is a well known fact that prolonged heating of certain polycyclic naphthenic hydrocarbons with hydrogenation catalysts in the presence of high-pressure hydrogen causes distinct changes in the properties of the products, thereby indicating conversion of possible isomeric forms into one another.

Waterman, Clausen, and Tulleners,[1] who studied the hydrogenation of naphthalene with nickel on kieselguhr, showed the conversion of *cis*-decahydronaphthalene into the *trans* form on prolonged heating of the hydrogenated starting material under the hydrogenation conditions. In hydrogenation experiments with anthracene similar conversions of the hydrogenation products occurred.[2]

It can be expected, that during hydrogenation of certain mineral oils, especially those of a pronounced cyclic character, conversions of the same nature will play a part. In this connexion attention should be paid to the observations of Lipkin, Martin, and Worthing,[3] in their hydrogenation experiments with certain highly aromatic lubricating oil fractions. They observed that prolonged heating of the nearly completely saturated oils caused a remarkable increase of the specific refraction, which was attributed to removal of traces of sulphur, oxygen, or nitrogen compounds in the last stages of the hydrogenation process. However, the increase of specific refraction and the considerable viscosity decrease can be more readily attributed to conversions of the *cis–trans* type (*cf.* Tables I and II).

TABLE I

Hydrogenation of Aromatics from Gulf Coast Oil According to Lipkin, Martin, and Worthing [3]

% oil hydrogenated *	d_4^{20}	n_D^{20}	r_D^{20}	Mol. wt.	Kin. visc., cs		V.I.
					100° F	210° F	
0	0·9976	1·5619	0·3233	325	634	13·3	− 279
83	0·9404	1·5062	0·3161	335	224	9·8	− 95
89	0·9365	1·5035	0·3159	335	220	9·7	− 98
90	0·9355	1·5029	0·3159	334	193	9·2	− 82
92	0·9347	1·5029	0·3162	339	204	9·5	− 82
95	0·9323	1·5017	0·3164	332	182	9·0	− 77
99+	0·9295	1·4999	0·3164	—	161	—	—
99+	0·9285	1·4995	0·3165	332	144	8·3	− 59
100	0·9283	1·5000	0·3168	330	151	8·3	− 67

* Based on interpolation between original and final density.

TABLE II

*Physical Constants of cis–trans * Isomers of Naphthenic Hydrocarbons* [4]

Compound	B.p., °C, 760 mm	n_D^{20}	d_4^{20}	r_D^{20}	Kin. visc., cs, 20° C
1,2-Dimethylcyclopentane :					
cis	99·532	1·42217	0·77262	0·32902	1·05 (15° C)
trans	91·869	1·41200	0·75144	0·33114	0·78 (15° C)
1,3-Dimethylcyclopentane :					
cis	91·725	1·41074	0·74880	0·33140	—
trans	90·773	1·40894	0·74479	0·33190	—
1-Methyl-2-ethylcyclopentane :					
cis	128·050	1·42933	0·78522	0·32853	0·92
trans	121·2	1·4219	0·7690	0·3304	0·77
1,2-Dimethylcyclohexane :					
cis	129·728	1·43596	0·79627	0·32834	1·39
trans	123·419	1·42695	0·77601	0·33083	1·04
1,3-Dimethylcyclohexane :					
cis †	120·088	1·42294	0·76603	0·33238	0·825
trans ‡	124·450	1·43085	0·78472	0·32976	1·10
1,4-Dimethylcyclohexane :					
cis	124·331	1·42966	0·78285	0·32975	1·12
trans	119·351	1·42090	0·76255	0·33249	0·926
Decahydronaphthalene :					
cis	195·69	1·4810	0·8965	0·3174	3·77
trans	187·25	1·4695	0·8699	0·3204	2·45
9-Methyldecahydronaphthalene :					
cis	215	1·4804	0·891	0·319	—
trans	205	1·4631	0·862	0·319	—
9-Ethyldecahydronaphthalene :					
cis	233	1·480	0·886	0·321	—
trans	225	1·466	0·861	0·322	—
1,10-Dimethyldecahydronaphthalene :					
cis	220	1·4812	0·8896	0·3200	—
trans	213	1·4659	0·8653	0·3208	—
	M.p.	n_D^{100}	d_4^{100}	r_D^{100}	
Perhydroanthracene :					
cis–cis	62	1·4810	0·9021	0·3155	2·60 (100° C) §
trans–trans	90	1·4637	0·8647	0·3193	1·68 (100° C) §

* Labelling *cis* or *trans* according to API Research Project 44; selected values of properties of hydrocarbons and related compounds, Carnegie Institute of Technology, Pittsburgh.

† Formerly labelled *trans*.

‡ Formerly labelled *cis*.

§ Determined in Technische Hogeschool, Delft, Laboratorium voor Chemische Technologie by J. Cornelissen.

* MS received 13 December 1954. † Technische Hogeschool, Delft, Laboratorium voor Chemische Technologie.

In this paper some isomerization experiments are described with hydrogenated Balikpapan and Tarakan lubricating oils and a medicinal liquid paraffin.

EXPERIMENTAL

Starting materials for the isomerization experiments were obtained by saturation (hydrogenation) of certain high cyclic lubricating oil fractions of Tarakan and Balikpapan origin (A and B, respectively). Hydrogenation was performed under relatively mild conditions (approx 300° C, 200 atm of hydrogen, equal weights of 20 per cent nickel on kieselguhr catalyst); complete de-aromatization of the oils, however, could not easily be reached. Several hydrogenated oil samples were used in a series of isomerization experiments, in which the samples were heated at higher temperatures (335°–370° C) with 10 to 20 per cent of nickel on kieselguhr during prolonged heating times (25 to 85 hr); in so far as the oil samples had not yet been fully saturated in the preliminary hydrogenation process, this reaction was completed during the isomerization experiments. Although therefore both reactions should not be considered quite apart, this is of no importance with respect to the results of the investigation. A survey of the several experiments and the analyses of the reaction products is given in Table III.

A third starting material in the investigations was a medicinal oil (liquid paraffin) M. Although, as a consequence of heavy acid and oleum treatment, medicinal oils can be expected to be entirely free from aromatics, a prehydrogenation of the oil was also carried out in this case (see Table IV).

Isomerizations were performed at 275° to 325° C and hydrogen pressures of approx 50 atm, at higher temperatures cracking of the oil became of importance. The results are collected in Table IV and represented in Figs 1 and 2.

TABLE III

Hydrogenation and Isomerization Experiments of Tarakan and Balikpapan Lubricating Oils

Initial pressure 100 atm (0° C) in all experiments

	Expt. No.	Starting material	Reaction temp, °C	Wt % of Ni catalyst	Reaction time, hr	n_D^{20}	d_4^{20}	Specific refraction $\frac{n^2-1}{n^2+2}\cdot\frac{1}{d}$	Mol. wt.	Spec. dispersion $\frac{n_G-n_C}{d}\cdot 10^4$	Kin. visc., cs 20° C	40° C	60° C	80° C	V.I.	*n–d–M* analysis % C_A	% C_R	R_A	R_T
	—	Tarakan Oil	—	—	—	1·5383	0·9632	0·3249	310	250	227	50·4	18·2	8·8	−125	32	67	1·2	3·2
Hydrogenation	A 1	,,	290	85 *	10 *	1·4983	0·9209	0·3185	300	162	119	37·5	14·1	7·4	−30	5	70	0·2	3·3
	A 2	,,	305	115	11	1·4970	0·9177	0·3189	305	165	90	28·2	12·3	6·7	−30	4	68	0·1	3·2
	A 3	,,	315	75	5	1·4941	0·9150	0·3182	295	156·5	98	29·6	12·7	6·9	−30	2	69	0·1	3·2
	A 4	,,	300	85	5½	1·4947	0·9167	0·3180	300	157·5	102	30·1	12·9	6·9	−30	1	69	0·1	3·3
	A 5	,,	315	85 *	9 *	1·4926	0·9130	0·3181	305	154·5	92	28·2	12·5	6·7	−30	0	67	0	3·2
Isomerization	A 51	A 5	335	11	25	1·4913	0·9102	0·3184	285	154·5	82	26·0	11·5	6·3	0	1	69	0	3·1
	A 41	A 4	350	11	25	1·4895	0·9059	0·3189	285	—	69	22·8	10·4	5·9	0	0	67	0	2·9
	A 11	A 1	355	10	25	1·4873	0·9013	0·3193	285	—	18·2 (100° F)		3·2 (210° F)		0	0	65	0	2·8
	A 52	A 5	355	10	25	1·4887	0·9042	0·3190	285	155	56	19·9	9·4	5·4	10	0	66	0	2·9
	A 53 †	A 5	355	20	85	1·4869	0·8999	0·3196	285	154	51	18·5	9·0	5·3	20	0	66	0	2·8
	A 54	A 5	370	14	25	1·4800	0·8834	0·3216	275	158	19	8·8	4·9	3·2	10	0	59	0	2·4
	—	Balikpapan oil	—	—	—	1·542	0·9649	0·326	360	—	982	165	48	19·5	−65	33	60	1·5	3·4
Hydrogenation	B 1	,,	315	25	4	1·5306	0·9514	0·3250	360	238	664	118	38	16·1	−65	27	59	1·2	3·4
	B 2	,,	295	80	4	1·5246	0·9444	0·3243	360	221	506	103	34	15·1	−40	24	59	1·1	3·3
	B 3	,,	300	80	6·5	1·5049	0·9219	0·3217	355	182	267	66	24	11·7	0	12	59	0·5	3·3
	B 4	,,	305	100	6	1·4939	0·9080	0·3202	365	160	202	53	22	10·5	0	3	58	0·1	3·3
	B 5	,,	310	100	26	1·4921	0·9069	0·3200	370	156·5	214	59	23	11·2	20	1	58	0·1	3·3
Isomerization	B 51	B 5	335	9	25	1·4886	0·8999	0·3205	355	156·5	139	41	17·3	9·1	20	1	56	0	3·1
	B 52	B 5	335	21	25	1·4862	0·8948	0·3210	345	154	104	34	15·1	8·1	45	0	55	0	2·9

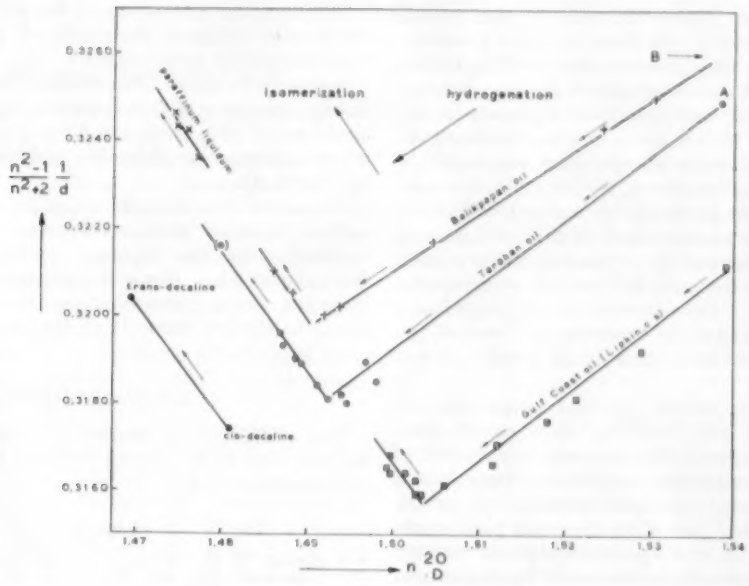
* Hydrogenated in two stages.

† After distilling off 12 per cent of the product (boiling point <275° C, 1 atm).

TABLE IV

Isomerization Experiments with a Hydrogenated Medicinal Liquid Paraffin

	Starting material	Hydrogenated starting material	Isomerization experiments with hydrogenated starting material				
Expt. no.	M	M 1	M 11	M 12	M 13	M 14	M 15
Reaction temp (° C)	—	200–250	295	275	295	275	300
Initial pressure (atm)	—	100	25	50	25	25	25
Wt % of Ni-catalyst	—	10	5	12	200	200	200
Reaction time (hr)	—	8	30	24	30	60	60
n_D^{20}	1·4770	1·4772	1·4770	1·4761	1·4759	1·4749	1·4746
d_4^{20}	0·8738	0·8734	0·8732	0·8701	0·8699	0·8680	0·8666
Specific refraction	0·3234	0·3236	0·3236	0·3242	0·3242	0·3243	0·3246
Mol. wt.	375	370	370	370	360	370	360
Kin. visc. (cs):							
20° C	87·0	86·4	84·3	71·3	71·3	65·2	65·1
40° C	30·8	30·7	29·3	26·4	26·6	24·6	24·4
70° C	10·4	10·4	10·1	9·35	9·32	8·85	8·77
V.I.	80	80	80	80	80	90	90
n–d–M analysis: % C_A	−1	−1	−1	−1	−1	−1	−1
% C_R	43	44	43	42	42	41	41
R_A	−0·1	−0·1	−0·1	0	0	0	0
R_T	2·4	2·4	2·4	2·3	2·3	2·3	2·2
% C	—	86·12	86·16	86·11	86·19	86·19	86·30
% H	—	13·86	13·82	13·85	13·84	13·78	13·71
R_T from mol. wt. and % H	—	2·2	2·3	2·2	2·0	2·3	2·4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0,3240
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0,3200
0,3180
0,3160
1,47
1,48
1,49
1,50
1,51
1,52
1,53
1,54
B
A
isomerization
hydrogenation
paraffinum liquidum
Balikpapan oil
Terakan oil
Gulf Coast oil (Liphin. c o)
trans-decaline
cis-decaline
n 20 D

FIG 1

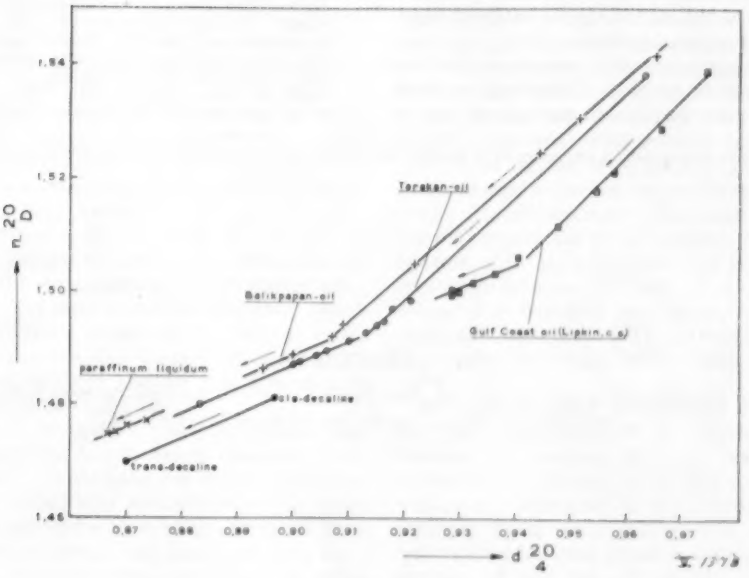
1,54
1,52
1,50
1,48
1,46
0,87
0,88
0,89
0,90
0,91
0,92
0,93
0,94
0,95
0,96
0,97
n 20 D
d 20 4
Terakan-oil
Balikpapan-oil
Gulf Coast oil(Liphin. c o)
paraffinum liquidum
cis-decaline
trans-decaline

FIG 2

DISCUSSION OF THE RESULTS

As follows from Tables III and IV the specific refraction of the several isomerized oils show a remarkable increase after they have reached the fully hydrogenated state. As a consequence R_T, the average number of rings per mol (calculated by means of the *n–d–M* method) shows an ostensible tendency to decrease. This, however, should not be ascribed only to cracking or decyclization of the oils. The changes in molecular weight generally are within the limits of accuracy of the determination (± 5 per cent), only at higher temperatures and longer heating times a small molecular weight decrease is observed (experiments A 53 and A 54). Decyclization is not very likely under the conditions of the experiments; besides, the ultimate analyses of isomerized liquid paraffin do not point that way.

The changes in certain physical properties, *e.g.* specific refraction and viscosity, show considerable resemblance to those in the *cis–trans* conversion of decaline under comparable conditions. Data in the literature concerning the hydrogenation of certain aromatic lubricating oils[3] show the same phenomena (*cf.* Figs 1 and 2). In all probability isomerizations of the *cis–trans* type play a part in the hydrogenation process of these strongly cyclic oils.*

The curves of Figs 1 and 2 show a very sharp transition point. This indicates that isomerization occurs chiefly after complete saturation of the starting material, the nickel on kieselguhr catalyst showing a large hydrogenation selectivity.

This is of importance with respect to several methods of analysis of mineral oil fractions in which hydrogenation of the fraction is performed; only a prolonged treatment of the saturated oils in the presence of high-pressure hydrogen and nickel catalysts causes irregular alterations of the physical constants which may influence the results of graphical statistical analyses to some extent.

Remarkable is the considerable viscosity decrease during the several isomerization experiments, this again being in accordance with phenomena of the *cis–trans* conversion of decaline and similar compounds (*cf.* Table II).

In general, the viscosity indexes of the oils show a distinct increase during isomerization. This holds especially for the stronger cyclic Tarakan and Balikpapan oils. However, the uncertainty of the viscosity index determination is relatively large owing to the low viscosity of the investigated oils.

ACKNOWLEDGMENT

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* However, other isomerization processes with analogous changes in the physical constants should not be excluded.

ERRATUM

DESIGN AND OPERATION OF A FLUID CATALYST PILOT PLANT FOR FISCHER–TROPSCH SYNTHESIS

Page 121, left-hand column, line 8: For "100 gal" read "10 gal."

OBITUARIES

SIR ANDREW AGNEW, C.B.E.

THE death of Sir Andrew Agnew has taken from our industry, not only in the U.K. but throughout the world, one of our most powerful and colourful leaders.

Although he was only seventy-three years of age, he had been a well-known figure in our industry for nearly half a century, first of all in the East and then for many years amongst us here in London. Members of the Institute of Petroleum will remember him as one of our most distinguished Presidents in the years 1946–48.

Sir Andrew was the very picture of the rugged Scot, and his actions did not belie his appearance, for he was at all times a forthright man of action.

Some of us knew him best as the Managing Director to whom we had to report on our return from periods of service abroad, for, as long as it was practicable, he made a point of seeing every member of his company as they returned from abroad for their period of leave. This visit to his office could be a very gruelling experience, because Sir Andrew went very thoroughly into the details of the happenings in our particular areas, and it was essential to know one's facts thoroughly if one hoped to deal adequately with Sir Andrew's questions. But at the end of the somewhat terrifying half hour, he would rise from his desk and, with a hearty handshake, would accompany us to the door of his office with his hand on our shoulders, bidding us good luck. The helpful advice which he gave amongst his questionings inspired one to return abroad with fresh vigour. In his own way he taught many of us good sound lessons on management.

Still others of us remember him best as the guiding hand in the war-time Petroleum Board. He came back from retirement to do this work, and no one else could have commanded the same respect throughout the industry. The remarkable success of our industry in fulfilling its war-time commitments owes much indeed to Sir Andrew's personality. I served under him in both capacities and learned to respect his knowledge of our industry and his wise approach to the varied problems as they arose.

His was a familiar figure in Shell-Mex House and in the Ministry of Fuel and Power in those war days. His urgent inquiry "What have you done?" will long be remembered by all who worked under him then, for he had little patience with meetings or committees unless some positive action had been put in hand.

Essentially a man of action, he could not bring himself to take a personal interest in long-term developments; and research, which inevitably called for patience, could not arrest his personal attention. He was prepared to leave this side of our industry to others. It is therefore all the more surprising to realize how much encouragement he gave to men of ideas and his sympathetic outlook towards them, although he could not await the results. His encouragement of Sir Harry Ricardo's work on diesel engines is one example of this broad-mindedness of Sir Andrew's.

His urgent tone seemed always to carry with it words of command. They have been missing among us, unfortunately, for some time, for Sir Andrew has been failing in health for two or three years; now, alas, we shall hear this vigorous voice no more, and our industry is the poorer for having lost one of its greatest men.

JOHN A. ORIEL

DR FRANZ KIND

ON 27 March, the petroleum industry lost one of its most prominent independent refiners. Dr Kind was the creator of the "Albatros" refinery in Antwerp (formerly "Redeventza"), the "Aquila" refinery in Trieste, the Manchester Oil Refinery in England, the "Condor" refinery in Milan, and the chemicals from petroleum works of Petrochemicals Ltd., Manchester.

Born in Austria in 1902, Dr Kind was the son of a man who himself was an independent refiner. He was introduced into the petroleum industry at a very early age, and his whole life was dedicated to the study and the promotion of petroleum technology. He obtained his Ph.D. in 1925 at Vienna University. When he reached the age of twenty-eight, he was already associated with refinery construction.

Dr Kind was an exceptionally gifted man, and his qualities were enhanced by prolific reading and intense work. He seldom made a decision without the most careful preparation of the subject, and after full discussions with anybody who had the ability and the interest to take part in them. In this way, he created around him teams of men keenly interested in their work, who were prepared to go to considerable lengths to acquire knowledge with which to support their chief.

Perhaps his most outstanding characteristic was his vision, and many of the developments which he foresaw came to pass, although when originally visualized they appeared to be highly imaginative. He was always interested in the economic conditions in different European countries, and in particular the relationship between those conditions and the development of the use of petroleum products. The various levels of industrial development in these countries represented, in his view, the variation in the demand made by each country. For this reason, he held the view that the logical development of the refining processes could take place only where it was possible for the refiners and their consumers to collaborate. These convictions made him a strong supporter

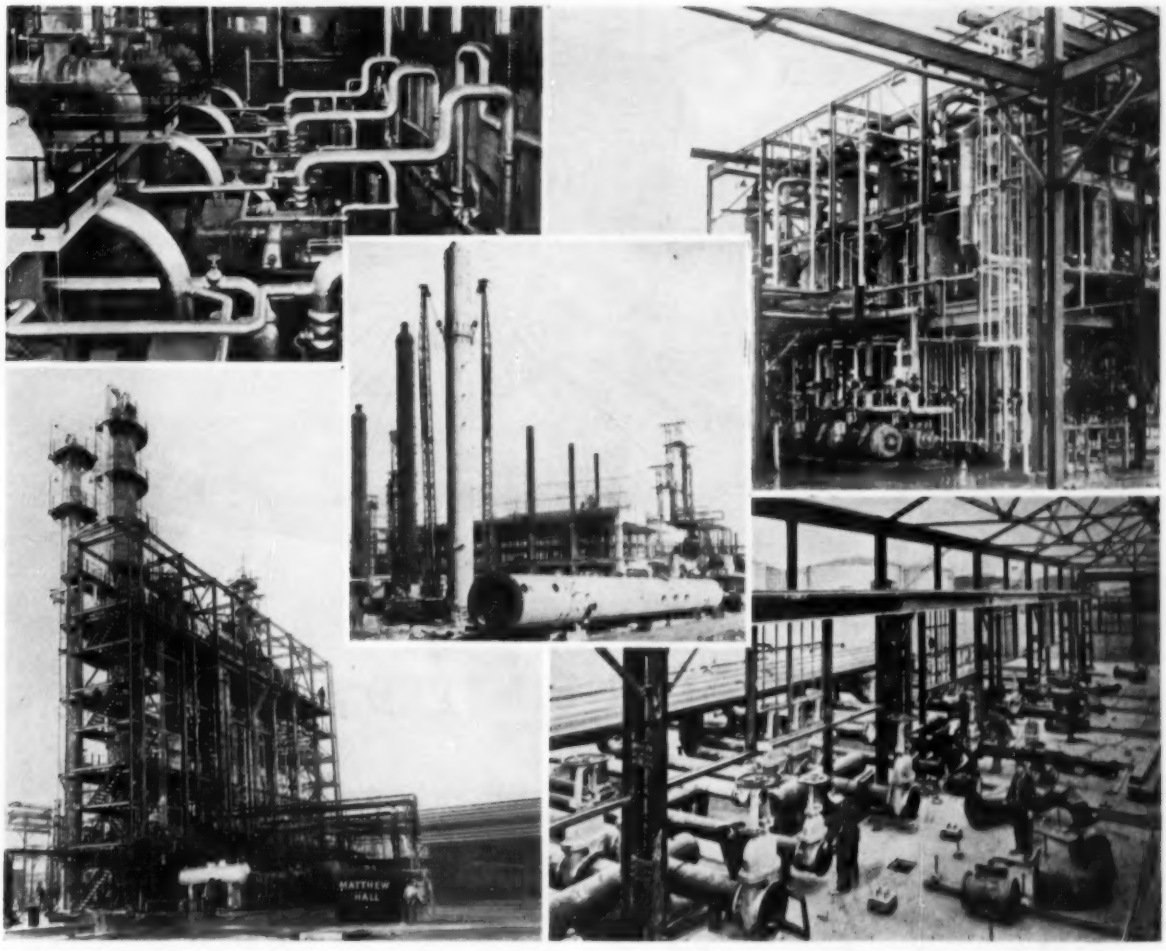
of home refining, at a time when this conception was not popular. Likewise he recognized at an early stage that petroleum provided the most adequate means to supply the needs of industry engaged in the manufacture of chemicals based on hydrocarbons.

Apart from his main interests, petroleum refining and production of chemicals from petroleum, Dr Kind was always interested in industrial processes in other fields, and took some pains to master the main principles which were involved, and to study the problems which presented themselves. As a result, his views on other people's problems were frequently sought, problems which he treated as seriously as if they were his own.

Probably another of his most outstanding features, with all his devotion to technical subjects, was his ability to maintain a warm personal relationship with all of whom he came in contact in his work. This led not only to a widespread feeling of admiration for his ability, but also to a sense of personal loss at his passing amongst the staffs and employees of the many organizations with which he was connected.

The petroleum industry today consists mainly of comparatively few large international units, in which the individual tends to be lost : this emphasizes still more that with Dr Kind's passing a light has gone out of the petroleum industry, and it is doubtful whether there can be found another man to play the part which he has played.

E. J. D.



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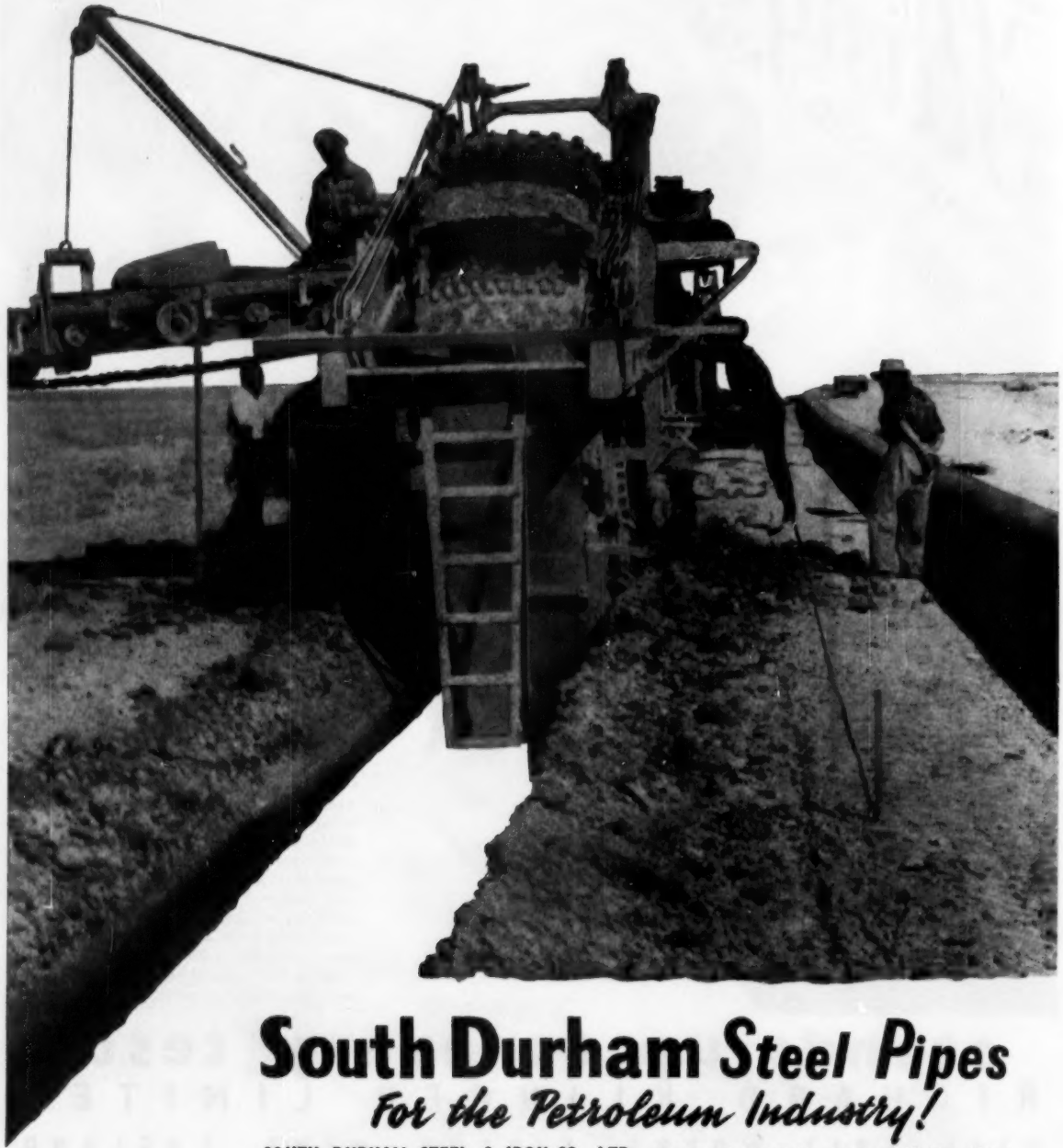

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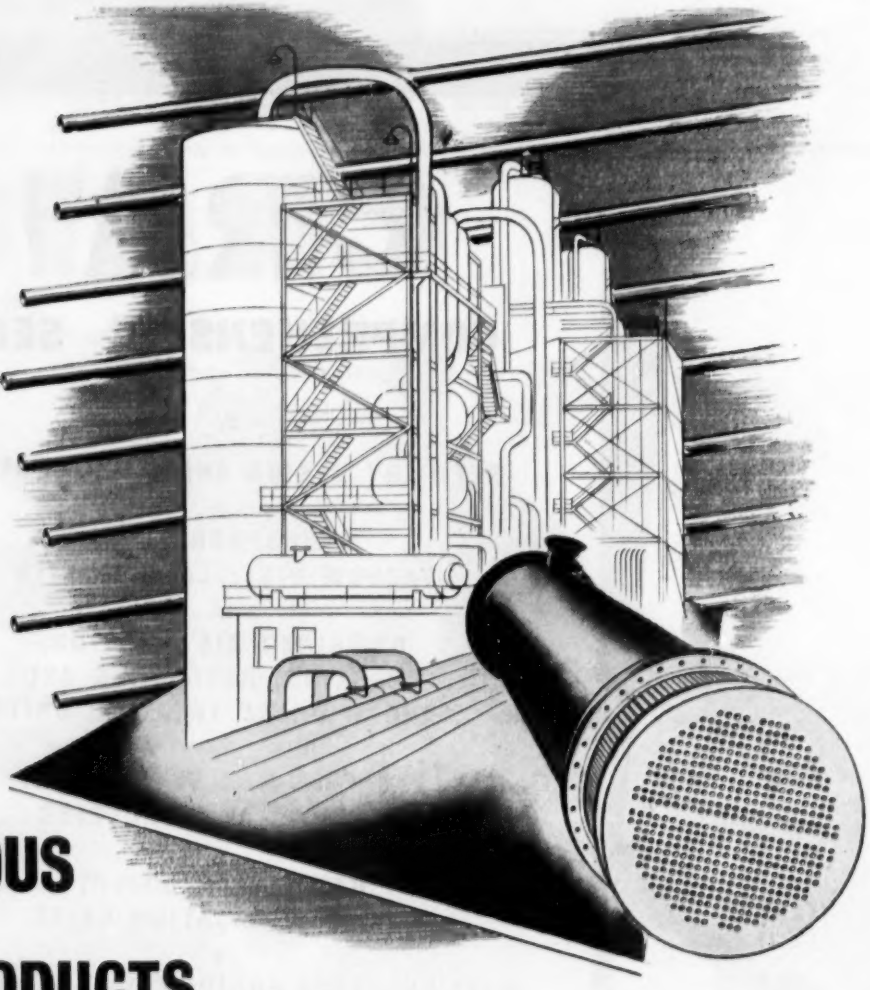

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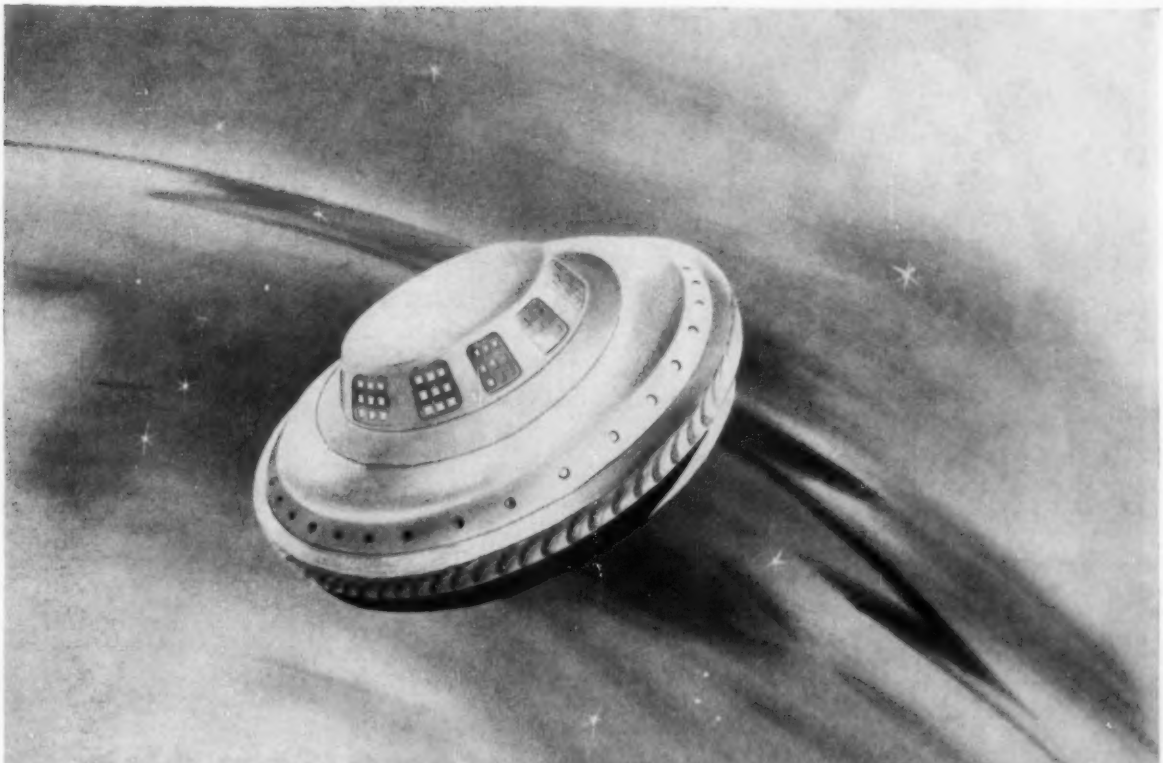

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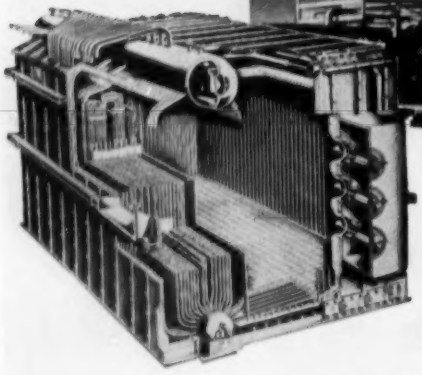

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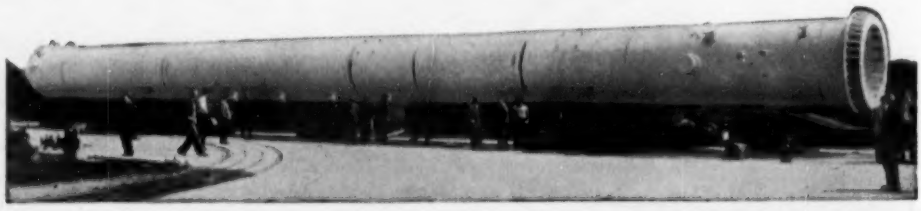

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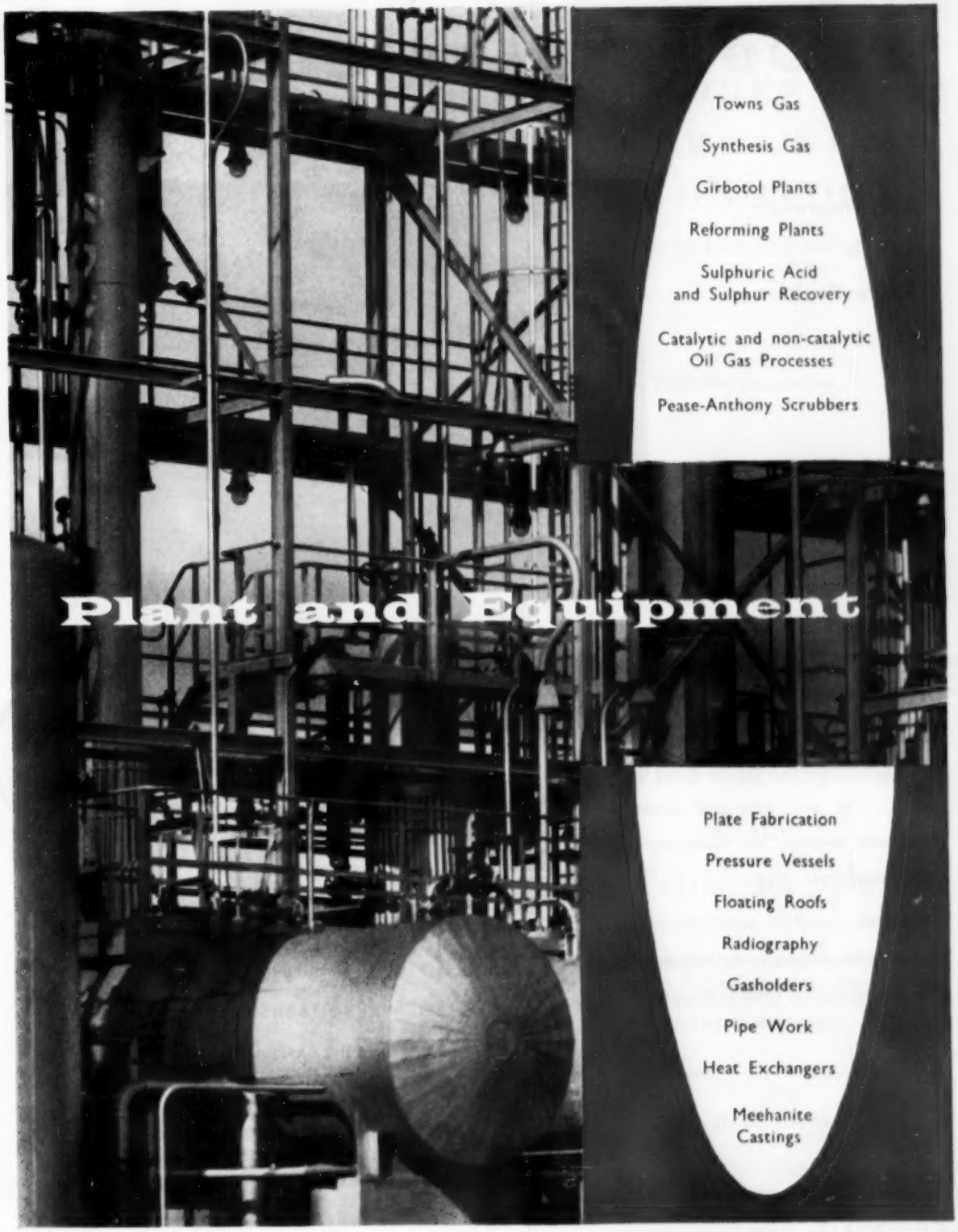

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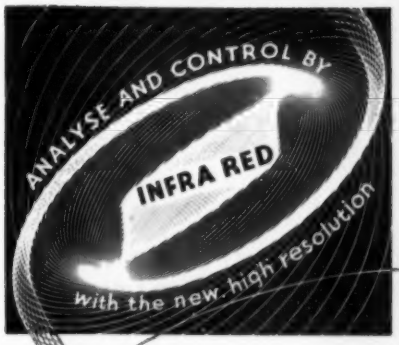

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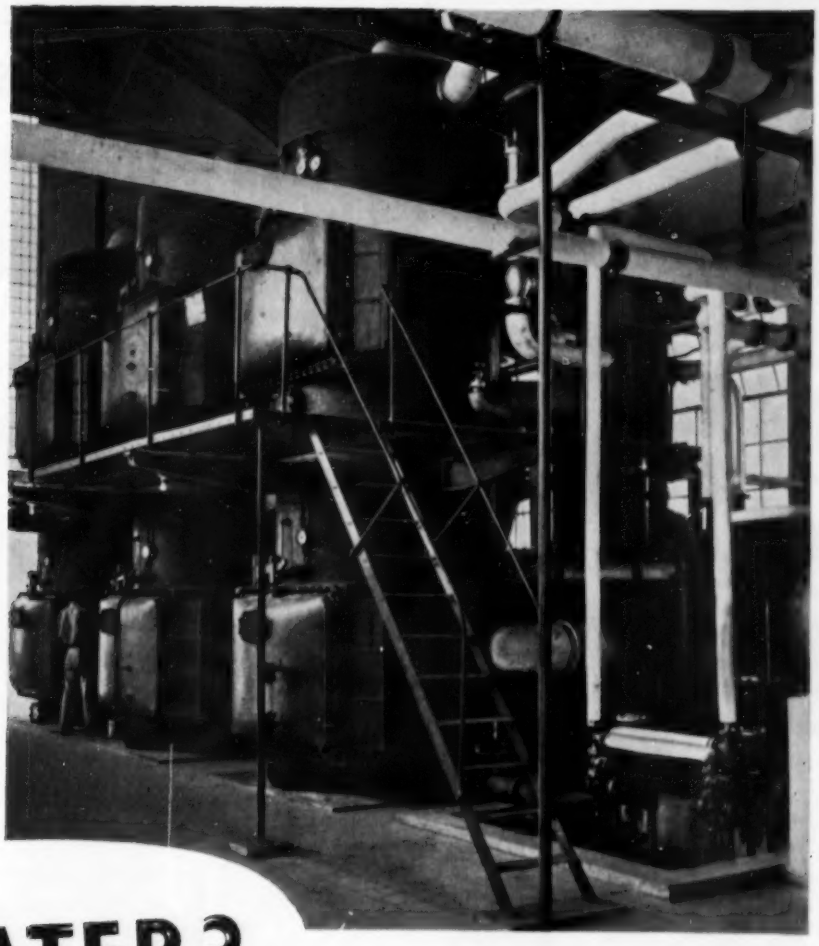

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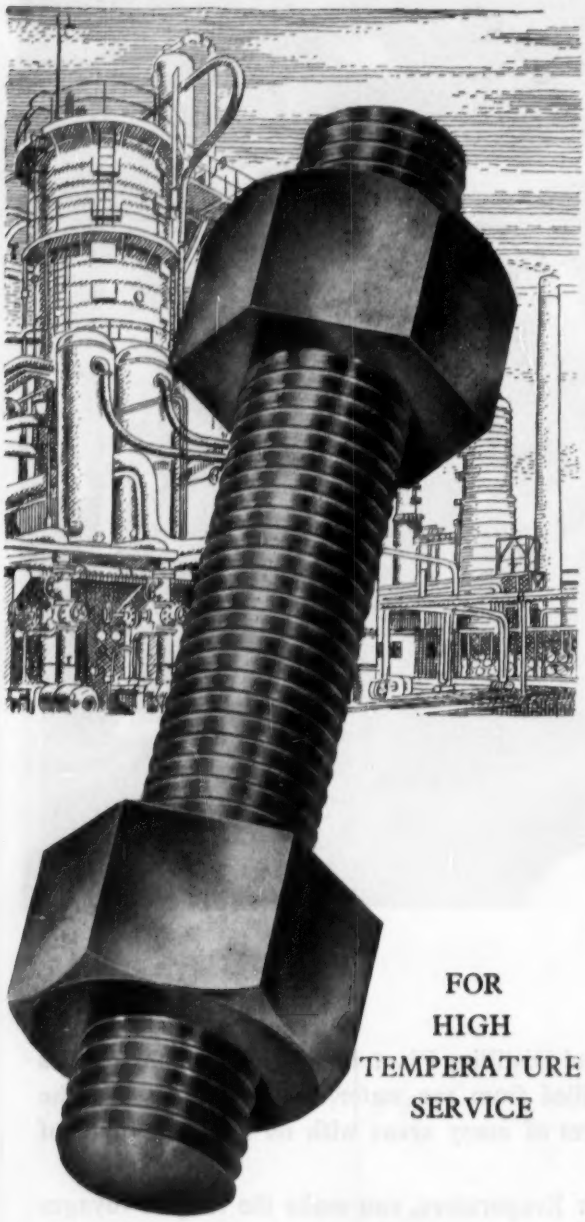

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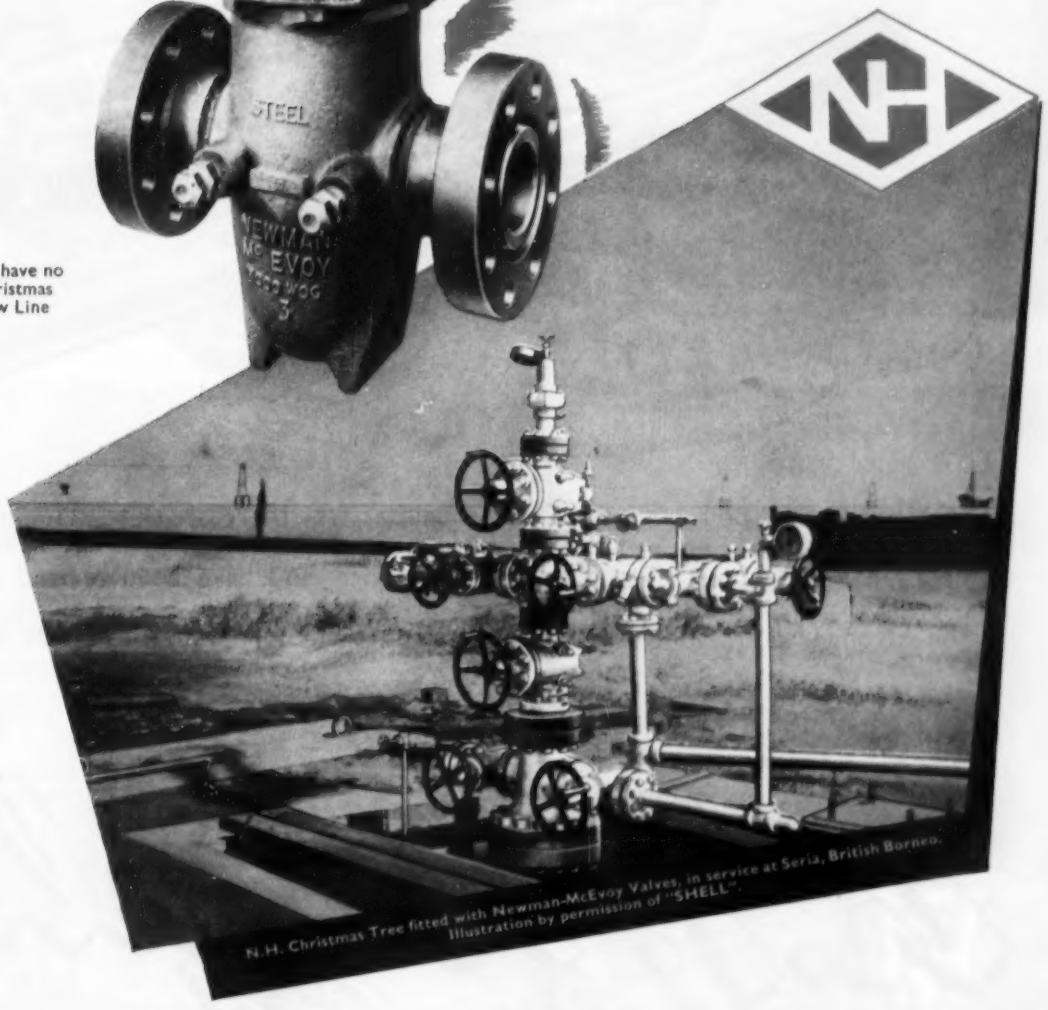

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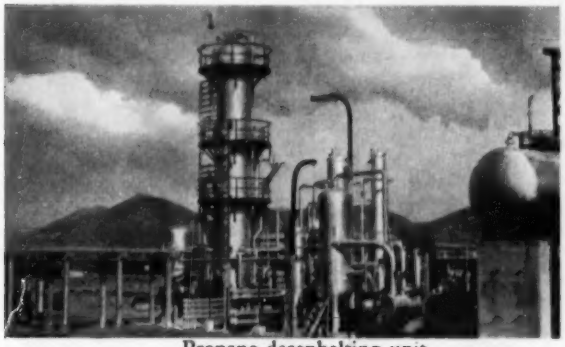

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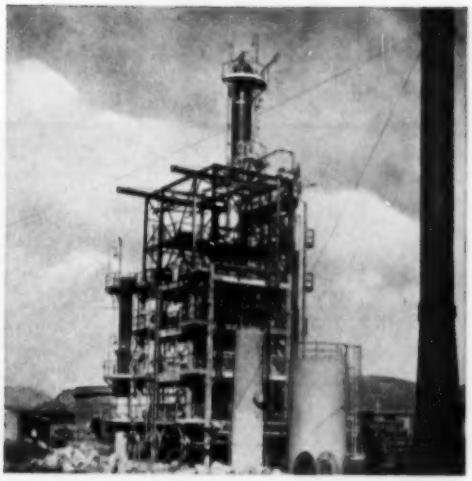
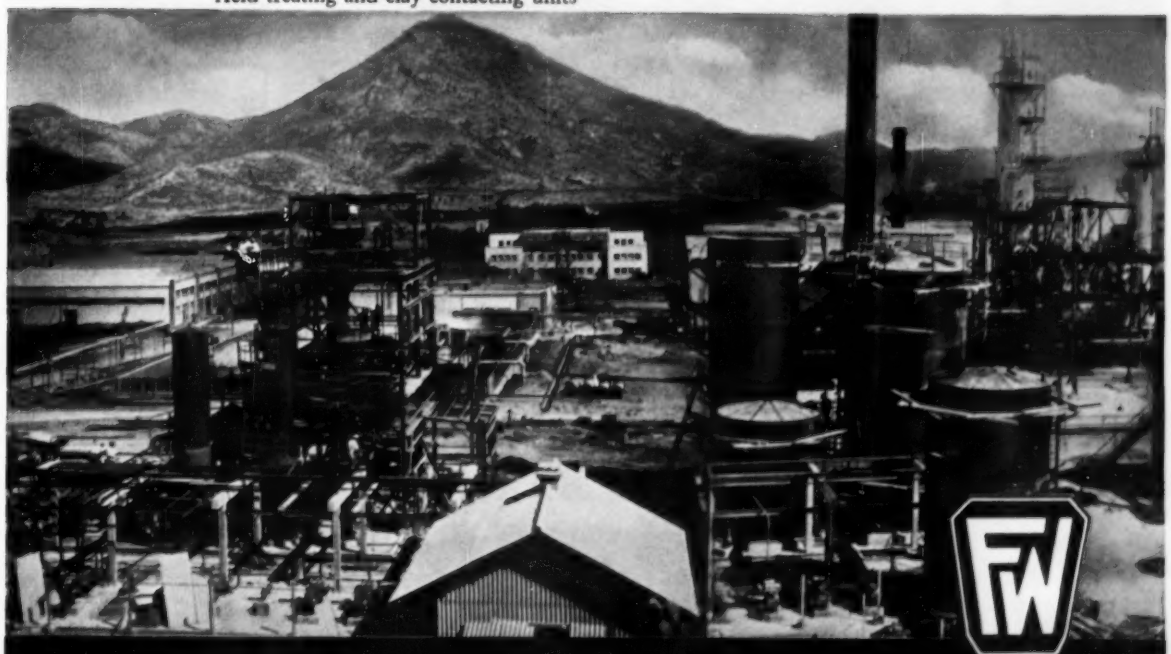
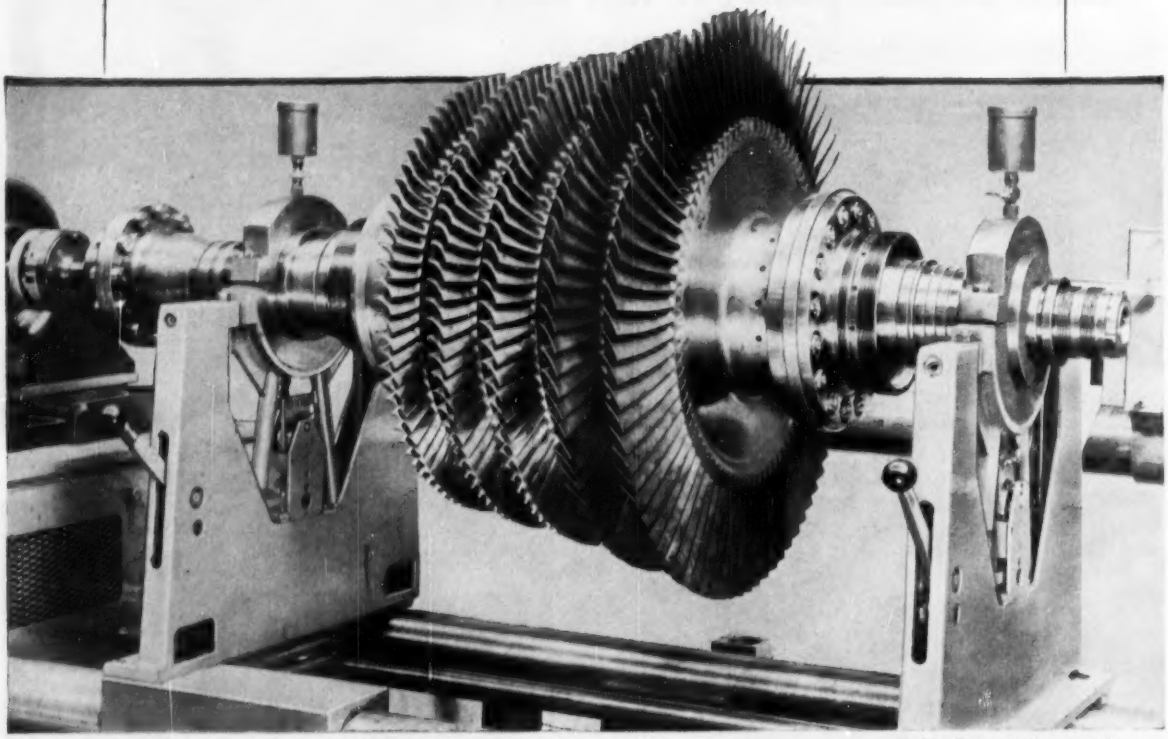


Furfural refining unit



FOSTER WHEELER *London New York and Paris*

Gas Turbines for Power Generation



Turbine Rotor in balancing machine

The 1,750/2,500 kW gas turbine Type L21 is one of a range manufactured by Metropolitan-Vickers for power generation. These robust and simple single-shaft machines have been built for standby and base load operation on liquid and gaseous fuels.

As leaders in gas turbine development, Metropolitan-Vickers would be pleased to give you advice and information.

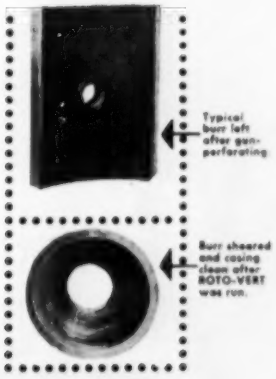
METROPOLITAN-VICKERS

ELECTRICAL CO LTD · TRAFFORD PARK · MANCHESTER, 17

Member of the A.E.I. group of companies

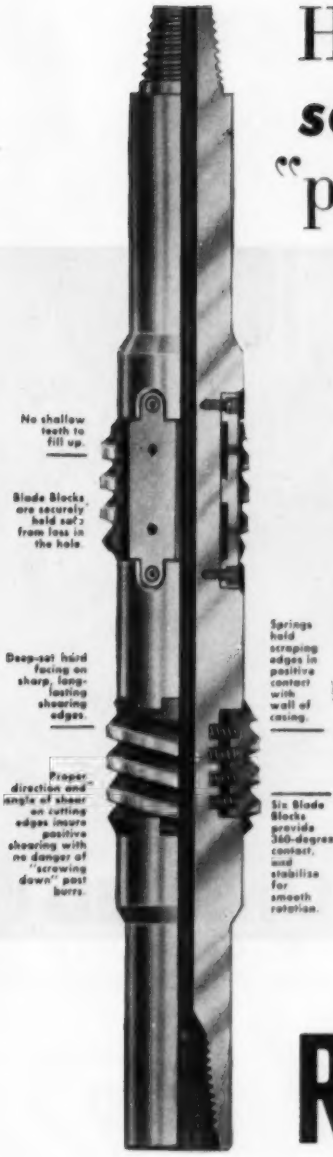
Progress in Power Generation

B/J401



Here's why

SCRAPING YOUR CASING "pays off"


No shallow teeth to fill up.
Blade Blocks are securely held safe from loss in the hole.
Deep-set hard facing on sharp, long-lasting shearing edges.
Proper direction and angle of shear on cutting edges insure positive shearing with no danger of "screwing down" past burrs.
Springs hold scraping edges in positive contact with wall of casing.
Six Blade Blocks provide 360-degree contact, and stabilize for smooth rotation.

Unless the inside of your casing—the vital "working surface"—is clean and smooth, there is always a possibility of trouble in future operations. Burrs from gun-shot perforations, hardened cement or mud, and even mill scale are all dangerous when the time comes to run testers or swabs, or to set retainers or packers of any description.

The Baker Model "C" ROTO-VERT Casing Scraper removes undesirable obstructions from the inside of the casing, leaving it clean and smooth. Burrs or imbedded bullets are sheared away; cement, mud and scale are scraped away, so that any down-hole work can be safely done at once, or during the years to come.

SCRAPE WHILE DRILLING OUT

The ideal time to use the ROTO-VERT Casing Scraper is during the drilling out of cement, at which time it is run as a "follow-up" behind the bit. Thus in a single run you can also remove all hardened cement, mud, burrs and scale which might later damage swabs or testers, and cause packers or retainers to set prematurely—or never set at all.

EASY TO RUN—LOW-COST RENTAL

The Baker ROTO-VERT Casing Scraper is simple to run, and will effectively clean the inside of the casing *when either rotated or reciprocated* (vertical action) The low cost is paid for many times over by freedom from future damage to bailers, swab rubbers and packers, as well as by the elimination of future delays and re-running expense.

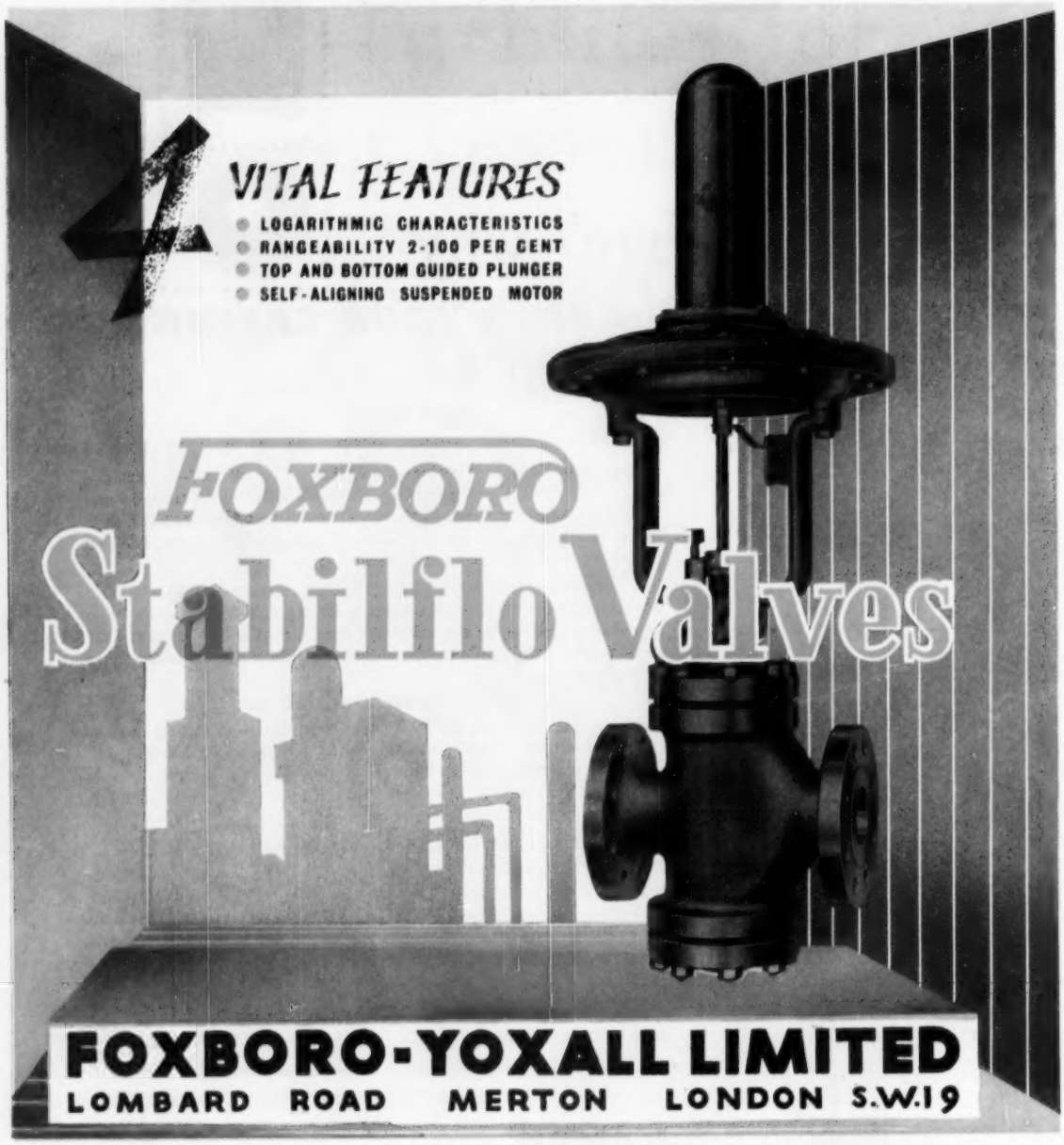
BAKER OIL TOOLS, INC.

Inquiries should be directed to...

P.O. Box 2274 Terminal Annex, Los Angeles 54, California, U.S.A.

BAKER ROTOVERT CASING SCRAPER

PRODUCT NO. 620-C


4
VITAL FEATURES
LOGARITHMIC CHARACTERISTICS
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LOMBARD ROAD MERTON LONDON S.W.19

Gas Turbine power plant on wheels

The current issue of the 'Wiggin Nickel Alloys' Bulletin contains an article on the STAL 2,400 kW rail-borne gas turbine power plant. Its use for the generation of electricity in the remoter parts of Sweden is described, also its construction, in which Nimonic* alloys are used for the high temperature parts.

*Registered trade mark.

Other features in this issue include :

ELECTRO-THERMAL DE-ICING

A GLASS-METAL SEALING PROBLEM

DETECTING AIRCRAFT FIRES

SUCTION PYROMETERS

News of gas turbine developments regularly appear in our periodical, sent free on request to all those who are interested. May we send you a copy ?

To Henry Wiggin & Company Limited, Publications Department, Wiggin Street, Birmingham 16.

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